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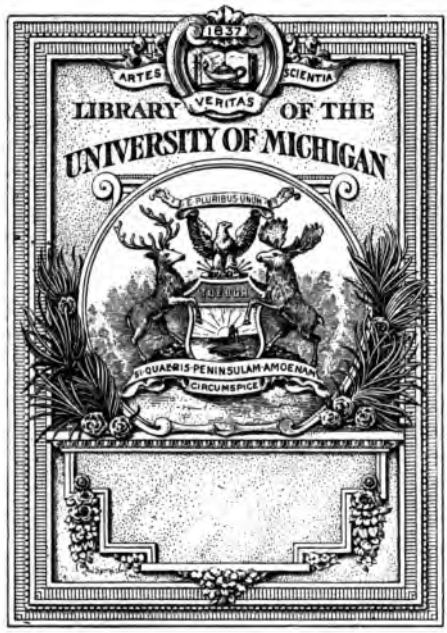
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MEMOIRS AND PROCEEDINGS

OF THE

MANCHESTER

LITERARY & PHILOSOPHICAL

SOCIETY.

(MANCHESTER MEMOIRS.)

VOLUME LII. (1907-8)

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NOTE.

The authors of the several papers contained in this volume are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

192932

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192932

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INAUGURAL ADDRESS.

By the President

Professor H. B. DIXON, M.A., F.R.S.

October 1st, 1907.

Two years ago our late President made an innovation which has caused me some anxious moments of reflection. By my action, perhaps, it would be decided whether it shall be the custom for our Society to demand of each new president an address, or whether we shall continue (as heretofore) unaddressed, and look on the late inauguration as something extraordinary and not to be repeated; regard it, in fact, as a somewhat dangerous overflow of mental activity—a sort of 'Spate' in the Highlands of literature and learning which form the happy hunting ground of my distinguished predecessor. But with the example of so many learned societies before me, and the happy precedent afforded by Sir W. Bailey, I thought, however inadequate my own share might be, that it would be to the advantage of our Society, if future Presidents gave us the benefit of their thoughts and experience.

At the annual meetings of the British Association, it was long the custom for the President to attempt a survey of the whole domain covered by the advance of science during the year. As one well-known President put it—"he took advantage of the elevation of his position . . . and casting his eyes round the horizon of the scientific world, reported to them what could be seen from his watch-tower." But this custom has fallen into disuse. Extreme specialisation in each province of science made it impossible that even the British Association could secure a succession of presidents ready to report to

their colleagues on what was happening round the whole horizon; and the modern president has usually spoken either of that portion of science familiar to him by his own work, or on that fruitful subject—the fate of the nation whose government, commerce, and education, are conducted by persons unchastened by research. The addresses of the Sectional Presidents, spoken to audiences of experts, have followed the lead of the Presidential address, and no longer attempt a survey of their respective sciences.

I would propose, then, in virtue of the brief authority vested in me by the Society, in the first place to speak shortly on some of the work recently done in chemical science, and especially that on the properties and reactions of gases at high temperatures, and secondly to make a few remarks on the character of our own proceedings.

The work on radio-activity has advanced chiefly on the lines of the disintegration theory, which has proved itself most valuable not only in suggesting the origin of radium itself, but in connecting together the series of substances arising from the spontaneous changes in the radio-elements. And if in the past we in Manchester have not taken the lead in radio-active researches, we can feel much confidence that no reproach on this score can be made against us in the future; for to-night we most warmly welcome Professor Rutherford as a candidate for our membership. The birth-place of the Atomic Theory need feel no disintegrating shock. The laws of chemical combination, the gas laws, isomerism, stereo-chemistry and other generalisations demand the chemical molecule and the atom. We have not got rid of Dalton's atoms, we are beginning to see how wonderfully they are constructed!

Another property of the chemical atom, valency, has recently been shown by the brilliant work of Messrs. Barlow and Pope to be connected in a most definite manner with the volume of the region dominated by the atom in crystalline structures. Following up this discovery, Le Bas has shown that the "spheres of influence" of carbon and hydrogen have the same volume throughout the whole series of solid normal paraffins—a remarkable confirmation of the theory. Professor Pope has brought before us a model of benzene, consisting of columns built up symmetrically of close-packed assemblages of spheres of influence, the latter having volumes proportional to the valencies of the atoms—carbon and hydrogen. He cut a slice off the column and partitioned off a piece of this in a hollow mould, and lo! a new "space" formula for the benzene molecule in which each hydrogen atom snuggled against three carbons, and each carbon atom touched three hydrogens. When we had got over the initial difficulty caused by the absence of bonds and our consequent wonder why the atoms stuck together, and when we saw that the isomerism of the benzene derivatives could be explained by this model as well as by the ordinary plane hexagon or by the wedge—we began to dimly recognise that we were in the presence of a great idea. In watching the manipulation of these close-packed assemblages I could not help recalling to mind Dalton's explanation of the reason why water expands when cooling from 4°C. to the freezing point. He pictured the ultimate particles of water with their 'atmospheres' as spherical, and conceived them, at the maximum density, to be piled up layer on layer like a *square* pile of shot—where each sphere touches four others in the same plane and rests upon four others in the plane below. Then, says Dalton, if the square pile is distorted

into a rhombus, each layer has its particles in a more condensed form, but raises the layer above so that the pile is increased in height, and the number of particles in a given volume becomes less.

We shall have now another answer to meet the sceptic's enquiry—"What do you know about your atom when it has combined with another?" I think it is a matter on which this Society may well feel pride that we were the first to hear this newest development of the atomic theory.

My reference to benzene will naturally recall to many here that the hexagon or ring of benzene has no monopoly among closed carbon chains. The researches of our member, Professor Perkin, on the formation and stability of various carbon rings are known and admired by all organic chemists. Of late years he has been specially occupied with researches on the Camphor and Terpene series, and his work has been crowned by the discovery—specially dear to the constitutional chemist—of processes for the synthetical preparation of Camphor and of Terpene in the laboratory. It is not out of place to mention here that, founded on Dr. Perkin's theoretical researches, a successful technical process for the manufacture of camphor from oil of turpentine has been developed in Manchester. Dr. Perkin and his pupils have also successfully investigated the exceedingly complex constitution of the natural colouring matters—Brazilin and Haematoxylin. This room is not, perhaps, the place for elaborate discussions on constitutional formulæ, but I feel sure the Society would be glad if Dr. Perkin brought before us an outline of his discoveries—as he makes them.

With regard to the work that I am most interested in

personally, M. Jouguet has recently published a series of elaborate mathematical memoirs on the propagation of the explosion-wave in gases. His data are taken from the work of Berthelot, Le Chatelier and my own. He tells us that we have made unjustifiable assumptions, and that we have proceeded by a sort of intuition rather than with the mathematic rigour necessary in such a research. I, for one, plead guilty. I may recall the fact that I found empirically that the rate of the explosion-wave in a number of gaseous mixtures was equal to the velocity of sound in *the burning gases* assumed to be at a temperature double that of ordinary combustion. I did not of course mean to explain the explosion-wave as being itself a sound-wave produced in the gaseous products of combustion, *i.e.*, a cause following its own effect; and I thought I had sufficiently guarded myself by calling my formula 'an empirical expression' which was found to be *so far parallel to the truth* that it might be useful as a "working hypothesis." I compared the explosion-wave to an intense compression-wave, propagated like a sound-wave by exchange of *momenta* on molecular collisions, and maintaining its intensity by reason of the chemical reactions set up. For instance, in the explosion of electrolytic gas, I imagined a steam molecule just formed in the wave front communicating momentum by collision to an unburnt hydrogen or oxygen molecule, and this in turn combining when it met an unlike molecule. According to this view the chemical reaction does not take place between cold molecules but between molecules half of which have been heated by collision with the products of combustion. There is a most remarkable agreement between the velocities given by my formula and the measured rates of the explosion-wave in a number of gases; and the use of this formula in the investigation

of the mode in which carbon compounds burn at high temperature has, I think, been justified. But who has not had to suffer that great tragedy of Science, as Huxley calls it, the slaying of a beautiful hypothesis by an ugly fact? In this case the increase of the specific heats of gases at high temperature is an ugly fact. I assumed it to be constant: my own experiments have convinced me that it is not. My formula, therefore, though parallel to the truth, is obviously not truth itself!

Again, I pointed out that the formula did not account for the curious fact that an increase in the initial temperature of the gases was accompanied by a diminution in the rate of the explosion. M. Jouguet's formula, founded on Hugoniot's equation, does account for this diminution. M. Jouguet argues that the heated products of combustion *follow* the wave front with great velocity, and I have abundant photographic evidence that this is the case: he states that the explosion-wave travels with the velocity of sound through this rapidly moving gas, and therefore has an absolute velocity (relatively to the unburnt gas) of both motions added together. The important question remains, can we calculate backwards by Jouguet's equations from the easily-measured rates of explosion to the unknown specific heats? M. Crussard* claims that this is an important new application. Apart from the consideration that Hugoniot's theory has not been experimentally established, I must point out that if we calculate backwards from the rates of explosion we get different values for the specific heats according as the condensation in the wave is assumed to be more or less. These values, however, lie between certain limits, and the method therefore promises to be useful. I must

* L. Crussard, "Ondes de choc et onde explosive." *Bull. Soc. de l'Industrie Minérale, Saint-Etienne*, 1907.

also point out that there is nothing novel in M. Crussard's idea of working backwards from the explosion rate. One of our members, Mr. D. L. Chapman,* has calculated the specific heats of gases from the explosion-rates by means of equations deduced from Riemann's formula for the propagation of a compression wave.

Until lately no one had measured directly the specific heat of a gas at temperatures beyond 200°C. The unexpectedly low pressures found by Bunsen in the explosion of gases, were attributed by him to the dissociation of the products, by Berthelot and by Le Chatelier to the great rise in their specific heats. Either hypothesis would account for the observed pressures. Dugald Clerk added a third explanation—that the gases combined comparatively slowly, and that heat was lost by conduction, etc., while the chemical combustion was still proceeding. The specific heats deduced from the pressure curves of exploded gases at high temperatures, did not appear on the prolongation of the curves drawn, for instance, for CO_2 or steam at low temperatures; hence the doubt as to what happens at high temperatures, and the necessity for making determinations at intermediate temperatures. The specific heat of air was determined up to 900°C. by a resonance method in an open tube by Stevens, but his results, which show a distinct rise, have been questioned by A. Kalähne,† who with an improved apparatus finds a much smaller increase.

I have directly determined the specific heat of CO_2 by heating the compressed gas in a steel bomb and dropping it into a specially constructed calorimeter. The rise in the specific heat is undoubted, but the experiments

* D. L. Chapman, "On the Rate of Explosion in Gases." *Phil. Mag.*, Jan., 1899.

† *Drude's Ann.*, xi., 225.

became difficult at 400° and the margin of safety was small. On the other hand the velocity of sound in CO_2 can be measured in a heated tube and compared with that in air or nitrogen under the same conditions; by this method I have found the rise in specific heat of CO_2 up to 400° on the assumption that air has a constant value. This experiment gives the specific heat *at* 400°C., and not, as in the cooling method, the mean specific heat between 20° and 400°C. Quite recently Holborn, at Charlottenburg, has succeeded in pushing Regnault's calorimeter method to high temperatures. I am glad to find that our curves for CO_2 run, as far as they go, nearly parallel with one another:—

Specific Heats of CO_2 at constant pressure.

	At 0°.	100°.	200°.	300°.	400°.	600°.	800°.
Holborn..	·203	·216	·228	——	·250	·268	·281
Dixon	·196	·208	·220	·232	·244	——	——

The rise in the specific heat from 0° to 100° agrees with the experiments of Wüllner; it is much less than that found in the old determinations of Regnault and of Wiedemann. The new values would "join up" fairly with the high temperature values deduced by Le Chatelier from the explosion pressures. Holborn, also, this summer has determined the specific heat of steam, and shown that it rises regularly. Dugald Clerk, too, by an entirely different method, has shown the rise in the specific heats of gases in the gas-engine cylinder.

There is, therefore, a satisfactory consensus of opinion as to the *fact* of the increase in the specific heats of the triatomic gases; the *rate* of the rise has still to be determined with precision.

Another point connected with gases that has recently occupied attention is the temperature of their ignition points. The methods that have been used, mainly on the

continent, do not avoid an error inherent in the method of heating the mixed gases together—viz., the gradual combination of the gases as they are raised to the ignition point. To obviate this difficulty, Falk has recently ignited the mixture by sudden adiabatic compression in a steel cylinder—and calculated the temperature from the volume occupied at ignition. I think he could not avoid a local cooling of the gas in contact with the cold walls of the cylinder and piston, which would affect his calculated ignition points. Mr. Coward has lately been working with an apparatus I designed with the aid of Mr. G. W. A. Foster, and has obtained ignition points which vary through a comparatively small range of temperature. The method consists in bringing the combustible gas through a small tube passing along the axis of a large tube, electrically heated.

When, five years ago, Dr. Edgar and I began to devise a method for burning a weighed quantity of hydrogen in a weighed quantity of chlorine, we thought our attempt—if it succeeded—would be regarded, at best, as an academic vindication of Stas' atomic weight. We did not anticipate that Richards in Harvard would be re-determining the chlorine-silver ratio, and Guye in Geneva and our countrymen, Scott and Gray, would be re-determining nitrogen, and comparing it with silver and chlorine. Silver was one of those few elements so exactly determined that we wrote its atomic weight with six figures, we knew it to a fraction of its ten-thousandth part! But the new determinations made it necessary either to alter silver or chlorine: hence the sudden importance of an independent determination of chlorine. Dr. Edgar has repeated our experiments in a different manner, and W. A. Noyes in America has made a fresh determination. Guye has also redetermined the density of hydrogen

chloride. I think the mean result will be close to our measurement, and that silver will come down in the world and suffer a depreciation of half a per cent.

In conclusion I should like to make a few remarks on some matters more directly concerning our Society and its proceedings. Our membership is not so large as it once was, and I have heard more than once in the last ten years dismal forebodings of imminent decay. We may have been too conservative towards new ideas, we may have been too slow to meet the changing conditions of the city's life, we may have leaned too much to philosophy and too little to literature; but I am convinced that as a scientific society there is nothing vitally wrong with us. Numbers count; we cannot carry on our usefulness unless new recruits are ready to join us, and every member should make it a matter of personal pride to introduce friends to our meetings. But numbers are not the first consideration; we must maintain and if possible increase the interest of our proceedings. I think we should have more papers down on our agenda, though I am not anxious to increase our printer's bill. We need not desire to be the sole channel of publication of communications made to us. Indeed I would ask our scientific workers to bring their discoveries before us as evidence of good-will, but, as editors say, *not* necessarily for publication. I would specially plead for the introduction, not of the corrected article, but of the rough proof; for the informal discussion of experimental ways and means; for science in the making. It is at this stage that new ideas, even crude ideas, falling on prepared ground, may be so fruitful. We need not be frightened of giving our valuable ideas away: it will be give and take, and besides, ideas are only valuable to those prepared to work them out.

It may be objected that more papers on the notice will crowd out or curtail the "short communication" on matters of scientific interest. I think I should not greatly regret if the short communications were crowded—not out, but, say, into a quarter of an hour. I would not urge the abolition of the 'short communication'—a result which would follow if the proposal were carried that the short-communicant had to give a day's notice to the secretaries. But when we see members, who have come to hear a particular paper announced on the summons, leaving the room during a long discussion on subjects adventitiously introduced as "short communications," then I think those members have a right to complain that they have been defrauded owing to our inability to perceive that one essential thing about the short communication should be its shortness. If I am right in this diagnosis, I shall look to the Society to support the chair in any restrictive measure that may be necessary

Those who have been connected with this Society for many years cannot help noticing the gradual change which has come over our *personnel.* One of our oldest and most devoted members, the late Dr. Schunck, in his farewell letter to the Society, deplored the increase of the professional element and the disappearance of the amateur. I would respectfully join in that regret. I suppose, as the sciences become more specialised, and as measurements become more refined and the instruments needed more costly, the amateur may feel that he cannot keep pace with those who devote their lives professionally to such studies in institutions equipped at the public expense or at that of the pious founder. There is some reason for this feeling; but I think there is none for another feeling which I believe exists—that professional men of science are jealous of the amateur, and by a

conspiracy of silence or contempt seek to obscure his merits and so maintain their own prestige. It is, of course, difficult for an amateur, unless he be a man of wealth and leisure, to improve on the measurements of our fundamental units. My experience, for instance, would not lead me to suggest to a business man anxious for some chemical recreation that he might with advantage re-determine the atomic weight of chlorine. But, on the other hand, no one can carry through a chemical research without meeting numerous problems in the shape of unexpected obstacles, most of which he has to 'side-track' if he wishes to make progress. And it is surprising what a little way one has to travel before bumping up against the unknown—or the forgotten. If anyone were to ask me the simple question, "Do hydrogen and oxygen combine slowly in the light?" I should have to say, "I don't know: the evidence is contradictory." Indeed, our ignorance is so vast that there are chemists who do not hesitate to affirm that in all probability nothing combines directly with anything. The study of the conditions under which some simple chemical change occurs would not, I believe, involve great expenditure of time or money; it would require patience, skill, and enthusiasm, it would require an open mind. Is not the amateur possessed of this armoury? I have spoken only of the experimental sciences: in astronomy, natural history, geology, does not the amateur hold his own?

Again, does not the amateur bring to our discussions a freshness of ideas which more than compensates for any possible defects of critical judgment? Criticism is cheap —ideas are precious. Ideas are given to the young—judgment is left to the old. And the amateur is always young! At an age when the professional has given up the game and become an umpire the amateur is still scoring!

There seems to me no reason in the nature of things why a professional in one science should not be a successful amateur in another—at all events be interested in its aims and development. And probably no more stimulating ideas are struck out than at the personal contact, if I may use the phrase, of two minds trained in different sciences. I know that I am very grateful for ideas received in this meeting-room, and I feel sure that many of our members feel similar gratitude. I would then plead for the cultivation of the amateur. I have tried to show how important he is to our Society: it must be our business to attract and then to interest him.

And there is another connected danger which I have heard threatens us. The Society is being swallowed up by the University. As the older Universities sent out "extensions" where the concentrated culture of higher intelligences was diluted down to the density of common comprehensions, so the Manchester University is seeking to establish a "centre" here where similar experiments may be carried out! Speaking as a member of that University I would only ask "Why should we?" We surely have enough lecturing to satisfy our professorial zeal: we are not ambitious to restart evening classes—unpaid. I think I speak for all my colleagues when I say that we come here because this Society knows no distinctions among its members, who meet on a perfectly equal footing whether professors or manufacturers, professional men or students; because criticism is informal and open to all; because it is advantageous that we should have mutual interchange of experience and ideas—especially between men of the laboratory and men of the works; and because it is our pride to maintain as best we may the tradition of this spot—hallowed by the fame of those achievements that know no wane.

Manchester Memoirs, Vol. lii. (1907), *No.* 1.

I. "On the Atomic Weight of Radium."

By HENRY WILDE, D.Sc., D.C.L., F.R.S.

(Received and read October 29th, 1907.)

In my paper read before the Society last year[1], it was shown from the relations of the specific gravities of the alkaline-earth metals to their atomic weights, and also from the similar relations of the series of alkaline metals, that radium would have a proximate specific gravity of 5, and an atomic weight of 184, notwithstanding the assertions that have been made that the new element would be a heavy metal comparable with thorium (sp. g. 11) and uranium (sp. g. 18), and that its atomic weight ranged between 225 and 258. It was also shown that the atomic weights of the two series Hn and H2n of my tables, are definite multiple differences *and not intermediate numbers.*

In the several accounts which have been given of the atomic weight of radium, it is stated that the experimental determinations were made with radium chloride. Now it is well known to chemists that the series of alkaline metals and alkaline-earth metals, magnesium, calcium, strontium, and barium unite with chlorine in one proportion only.

An important gain to chemical science which the multiple differences of the atomic weights have led up

[1] *Manchester Memoirs*, vol. 51, No. 2, 1906.

November 21st, 1907.

to, is the quantitative determination of the combining proportions of new elements in anticipation of the experimental results.

Hn		H2n	
	H = 1		He = 2
	Diff. − 6		Diff. − 6
0 . 0 . 7	Li = 7	0 . 0 . 8 = Gl =	8
	0·59† 7*		1·64? 9·2
	− 16		− 16
1 × 23 . 0 =	Na = 23	1 × 24 − 0 =	Mg = 24
	0·98 23		1·74 24
	− 16		− 16
2 × 23 − 7 =	Ka = 39	2 × 24 − 8 =	Ca = 40
	0·86 39		1·58 40
	− 23		− 24
3 × 23 − 7 =	Cu = 62	3 × 24 − 8 =	Zn = 64
	8·9 63·3		7·2 65
	− 23		− 24
4 × 23 − 7 =	Rb = 85	4 × 24 − 8 =	Sr = 88
	1·52 85		2·54 87·5
	− 23		− 24
5 × 23 − 7 =	Ag = 108	5 × 24 − 8 =	Cd = 112
	10·6 108		8·69 112
	− 23		− 24
6 × 23 − 7 =	Cs = 131	6 × 24 − 8 =	Ba = 136
	1·88 132		3·75 137
	− 23		− 24
7 × 23 − 7 =	— = 154	7 × 24 − 8 =	— = 160
	12·2‡		10·13‡
	− 23		− 24
8 × 23 − 7 =	— = 177	8 × 24 − 8 =	Ra = 184
	2·2‡		5·0‡
	− 23		− 24
9 × 23 − 7 =	Hg = 200	9 × 24 − 8 =	Pb = 208
	13·6 200		11·44 207

* Accepted Atomic Weights. † Specific Gravities. ‡ Estimated.

Taking the instance of radium chloride, I have estimated its combining weight after the classical method of Marignac[1] and Dumas[2] in their experimental determination of the atomic weight of barium as follows:—

1. One part of silver corresponds to 1·176 parts of radium chloride, or $\frac{1·000}{1·176}$.
2. The atomic weight of silver being 108, we have radium chloride = 1·176 × 108 = 127 minus Cl (35) = 92 the combining weight of radium with chlorine.
3. Now 2 × 92 = 184 is the bivalent atomic weight of radium with bivalent oxygen in the positive and negative series H2n, as shown in my general Table, with a possible increase of one unit in the experimental determination, as in the instance of barium (136—137).

I have previously shown that the positive series of elements H2n closes with lead (208), and that if any higher member of the series of alkaline-earth metals exist, it would have an atomic weight of 232, and an approximate specific gravity of 7.[3]

Assuming this hypothetical member to be radium, the combining equivalent of its chloride with silver (Cl 35 and Ag 108) would be 1·399 in accordance with the determinations arrived at with the other members of the same series, and not 1·371 as determined experimentally for the intermediate atomic weight 226, recently assigned to radium.[4]

[1] *Bibl. Univ. Archives*, 1858, p. 81.
[2] *Ann. Chim. Phys.*, vol. 55, p. 139, 1859.
[3] *Manchester Memoirs*, vol. 51, No. 2, 1906.
[4] *Comptes rendus*, vol. 145, p. 422, 1907.

Manchester Memoirs, Vol. lii. (1907), *No.* 2.

II. New Reactions for the Characterisation of Mercerised Cotton.

By JULIUS HÜBNER, M.Sc.Tech., F.I.C.

(Read and received November 26th, 1907.)

Although thoroughly mercerised cotton fibres exhibit very distinct microscopic characteristics (Hübner and Pope, *Journal Soc. Chem. Ind.*, Vol. XXIII., p. 404), on examining fibres taken from mercerised fabrics it will be found exceedingly difficult, in many instances, to say with certainty whether the goods have been mercerised or not. This may be due to incomplete penetration of the fibre by the soda solution, to the application of certain finishing operations after mercerising, and to other causes.

So far no reliable chemical reaction for the characterisation of mercerised cotton is known.

The author has found on immersing mercerised and ordinary cotton in a solution of iodine in saturated potassium iodide solution for a few seconds, and afterwards washing with water, that the colour of the mercerised cotton quickly changes to a blueish-black whilst the ordinary cotton becomes lighter in colour and changes to a brownish-chocolate shade. After further washing the ordinary cotton becomes white whilst the mercerised material remains a blueish-black colour, which fades very slowly on prolonged washing. The reaction proceeds still more distinctly and more slowly if, in place of water, a 2 per cent solution of potassium iodide in water is used for washing the cotton after immersion in the reagent. It will then be noticed that after 5 or 6 washings the

mercerised cotton is of a brownish-black shade whilst the ordinary cotton appears practically white. If now water is added the colour of the mercerised cotton changes immediately into a blueish-black the ordinary cotton remaining white.

It is well known that cotton treated with caustic soda solution exhibits increased affinity for the substantive cotton colours and that a considerable contraction takes place during this treatment. Hübner and Pope (*loc. cit.*) have shown that both the maximum absorption of colour, and the highest shrinkage of the fibres result from mercerising the cotton with caustic soda solution of from 50 to 60° Tw. They have also pointed out that the ratio of colour absorption and of shrinkage is not directly comparable with the increase of the strength of the caustic soda solution.

On applying the reagent described above to cotton mercerised with caustic soda of different strengths, it has been found that a distinct gradation of colour, directly comparable with Hübner and Pope's results, is produced.

On exposing the blue-coloured cotton, after washing, to the air, it will be noticed that on drying the colour fades gradually, and the more slowly the stronger the caustic soda which has been used in the mercerising of the material. If the cotton, after immersion in the reagent, is either not washed at all, or washed with potassium iodide solution, the brown colour of the ordinary and of the weakly mercerised cottons is seen to fade rapidly and very completely on drying, whilst the more strongly mercerised cottons retain the iodine for a very long time. Some of the samples have not faded after an exposure of six weeks.

The action of salts other than potassium iodide is at present under observation.

The author has ascertained that cotton mercerised either with or without tension possesses the property of absorbing iodine from solution in potassium iodide and water much more freely than does ordinary cotton. Cotton mercerised under tension absorbs less iodine than that mercerised without tension. A parallel between the rate of absorption of colouring matters and of iodine by mercerised cotton has, therefore, been established.

If ordinary and mercerised cotton is immersed in 20cc. of water to which two drops of a solution of iodine in potassium iodide, corresponding to about 0·0009736 gram of iodine, have been added it will be noticed that both samples are coloured very faintly yellowish. The author has found that, if, in the place of using water, aqueous solutions of zinc chloride are employed, the minute quantity of iodine present exhibits quite a remarkable action on the cotton fibres. The strength of coloration of the cotton increases to a certain point with the increase in the strength of the zinc chloride solution; the shade of the colour alters, and the difference in the strength of coloration between the mercerised and the ordinary cotton increases also with the strength of the zinc chloride solution until 100cc. of solution contains 93·3 grams of zinc chloride. At this concentration the ordinary cotton remains practically white whilst the mercerised cotton appears a dark navy-blue colour.

Cottons which have been mercerised with different strengths of caustic soda solutions show with this reagent also a gradation in colour by means of which the degree of mercerisation of a given sample may be ascertained.

The action of both reagents described above, on cotton treated with mercerising agents other than caustic soda, has also been investigated. The action of sulphuric

acid, phosphoric acid, etc., in place of zinc chloride in the last named reagent has also been studied.

Various inorganic salts and organic compounds, in conjunction with iodine, also give typical reactions on cotton ; the author is therefore extending his investigation to these.

It should be pointed out that these reagents may prove of value in the distinction of other textile and paper-making fibres, the various artificial silks, etc.

The author hopes shortly to lay the results of these investigations before the Society.

Manchester Memoirs, Vol. lii. (1908), *No.* **3.**

III. "The Cone of *Bothrodendron mundum* (Will.)."

By D. M. S. WATSON, B.Sc.,

Research Student in the Geological Department, the Victoria University of Manchester.

(*Received and read, November 12th, 1907.*)

In 1880, Williamson described a small hermaphrodite lycopodiaceous cone from a longitudinal section of a specimen obtained from one of the coal balls of the Halifax Hard Bed. He had previously described in detail the large and very characteristic macrospores and isolated microsporangia.

In 1893 he figured a portion of the cone not shewing the axis.

Solms Laubach ('**91**) briefly refers to this description of Williamson's, and states in addition that "the structure of the axis is essentially that of the type of *Lepidodendron Harcourtii*" (p. 237).

This description probably means nothing more than that the axis is Lepidodendroid and has a pith. I believe that Williamson was unacquainted with a transverse section of the axis, so that this description of Solms Laubach's may be founded on other sections, possibly those in the Cash collection.

These are, so far as I know, the only references to this type of cone; probably the rarity of whole cones and their usual bad preservation have discouraged investigators.

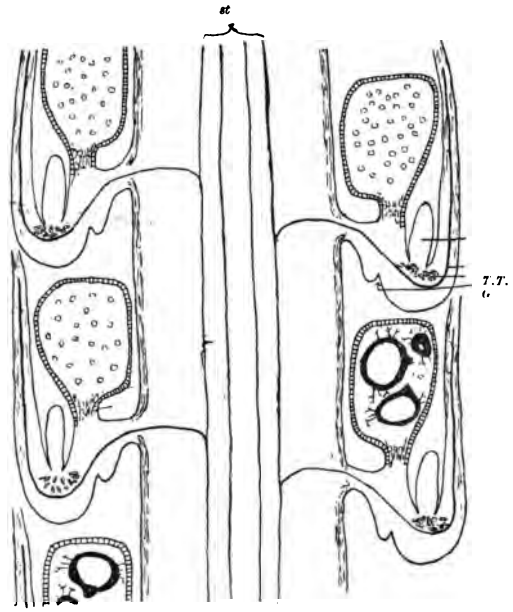
Only five individual complete cones are known to me, but isolated sporangia and spores are very common.

January 7th, 1908.

Microsporophylls have been erroneously identified as *Miadesmia.*

I am now able to give a fairly complete account of the anatomy and morphology of this type of cone, and refer it to *Bothrodendron mundum*, the *Lepidodendron mundum* of Williamson.

The material investigated consists of the sections Q454, Q458, Q460, of the Cash collection of the Manchester Museum, which form a series and are derived



Text-fig. 1.

Diagrammatic median longitudinal section of a cone of *B. mundum* × about 12.

lg=ligule. *vb*=vascular bundle of sporophyll.
st=stele of axis. *G*=the characteristic groove.
T. T.=transfusion tissue.

from the Halifax Hard Bed, A188 of my own collection derived from the mountain 4 ft. coal at South Grain,

Dulesgate, and A204 (my collection) from a coal seam in the second grit at Laneshaw Bridge, collected by Mr. P. Whalley. These sections are all transverse sections of whole cones, there being two in section A188. No really good longitudinal section of a whole cone exists, that figured by Williamson being badly preserved.

Sections R406 and Q461 in the Manchester Museum are figured in this paper, and many other sections shewing isolated sporophylls have been used.

Description of Cone.

General Morphology.

Except for the presence of a large and very conspicuous ligule, the cone much resembles that of *Lycopodium* in general structure (see *Text-fig.* 1).

It consists of an axis bearing short sporophylls composed of a small horizontal limb and a lamina on the surface of the cone.

On the upper surface of the horizontal portion, and about half-way between the axis of the cone and the lamina, a single sporangium is attached.

Between the attachment of the sporangium and the lamina is a large ligule set in a deep ligular pit.

Each sporophyll receives a single vascular bundle from the axis.

Size of the Cone.

In a compressed state, and with the appendages much disarranged, three transverse sections of the cone measure 10, 5, and 2 millimetres in greatest diameter. These differences are correlated with corresponding differences in the diameter of the axis which measures in the first two cases 3 and 2 millimetres in diameter respectively.

Taken in the light of the great tapering shewn in Williamson's longitudinal section, these figures suggest that the cone tapers rapidly.

It was probably about 8 millimetres in diameter at its widest place when uncrushed, and tapered to 3 or 4 millimetres at the top.

There is no good evidence as to its extreme length, but Williamson's longitudinal section, which includes the tip, shews macrospores at its base and microspores in the superior sporangia: it is thus probably a nearly entire specimen, and its length is 10 mm. approx. The cone was thus of very small dimensions when compared with the majority of the Lycopod cones of the Coal Measures.

Structure of the axis.

The wood is hollow and surrounds a pith of ordinary soft parenchymatous cells; this is as a rule badly preserved and is of no great interest. The wood is of comparatively large size when compared with that of an ordinary *Lepidostrobus* (See *Fig.* 1 of *Plate*). It consists of an inner layer one tracheid wide of large tracheids, this is succeeded by a belt of much smaller tracheids.

This belt exhibits a tendency to be split up into distinct groups, each of which encloses a protoxylem. The protoxylem points do not project, but form a quite smooth surface to the wood.

The whole wood very closely resembles that of a small twig of *Lepidodendron* (=*Bothrodendron*) *mundum*, Will.

In one cone on section A188 a small quantity of secondary wood has been developed in an irregular manner; this is seen in *Fig.* 2 of *Plate.*

Secondary wood is not yet known in stems of *Bothro-*

dendron; all those so far obtained are, however, quite small, only about 30 mm. in diameter. It is almost certain that the large stems, well known as impressions, had secondary wood.

The cortex in all sections known to me is very badly preserved; it seems to consist mainly of a soft tissue of large parenchymatous cells.

It is bounded externally by a narrow belt, 2 or 3 cells thick, composed of squarish cells, with dark, thick walls (See *Fig.* 2 of *Plate*).

Appendages.

All the appendages, except some of those at the top, and presumably also some at the bottom of the cone, are sporophylls.

Each sporophyll is attached directly to the axis, and its horizontal position projects about 6—8 mm. in its natural condition.

The sporophyll is attached to the cone by a very narrow attachment (See *Fig.* 3 of *Plate* and *Text-fig.* 1).

The very small area of this attachment accounts for the rarity of whole cones and the great abundance of detached sporophylls.

The horizontal portion of the sporophyll soon expands considerably.

There are really two tangential ridges across the under surface of sporophyll separated by a deep groove; the outermost of these is very large and receives a loop of the vascular bundle, it also serves to increase the block of tissue in which the ligular pit is excavated (See *Text-fig.* 1, and *Fig.* 3 of *Plate*).

The horizontal portion is composed of a small-celled parenchyma coated by a tissue of harder and more rectangular cells; a definite epidermal layer seems to

be present. This harder tissue tends to become more abundant towards the lamina of the sporophyll.

Passing through the sporophyll is a single vascular bundle; this is very small but seems to be collateral and orientated in the normal way. Its course is very simple, emerging from the cortex of the stem it runs directly through the leaf base, looping downwards at the exterior of the horizontal portion, and then passing upwards into the lamina of the sporophyll (See *Fig.* 3 of *Plate*). It is surrounded, particularly on the upper side beneath the ligule, with a great deal of tranfusion tissue of the type common in the Lepidodendraceae; this tissue in the neighbourhood of the ligule nearly cuts out the ordinary parenchyma of the leaf base.

The sporangium is attached by quite a narrow neck of tissue to the upper surface of the horizontal limb at about the middle of its length, this neck is roughly circular in transverse section, and is about as high as wide, it forms a pedicel on which the sporangium is seated (See *Figs.* 3 and 4 of *Plate*). The pedicel contains no vascular tissue. Both micro- and megasporangia are attached in the same way.

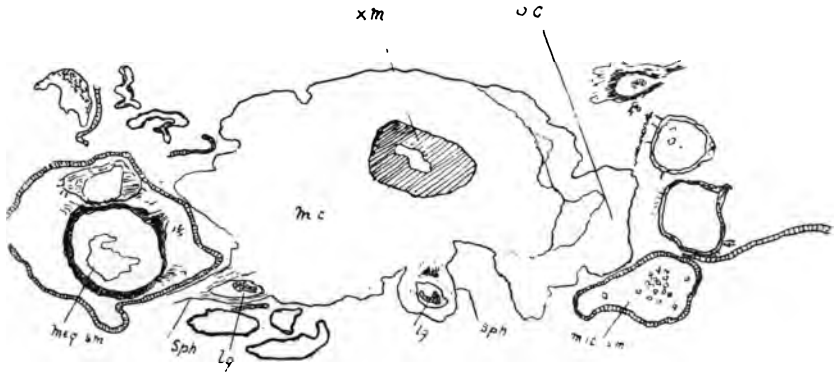
The pedicel expands into the sporangium with the wall of which its epidermis is continuous. The central tissue of the pedicel forms a little cushion inside the sporangium, and spreads out to form a lining layer to its lower part.

The sporangia are higher than they are wide either radially or tangentially.

The sporangial wall usually consists of a single layer of cells which are square in section, but irregular equidiametric polygons in surface view; they do not shew any trace of the buttresses which occur in Calamitean and sphenophyllaceous fructifications.

The ligule is inserted in a deep depression in the upper surface of the sporophyll, between the upturned lamina and the attachment of the sporangium ; this space it entirely fills (See *Text-fig.* 2 and *Figs.* 1 and 3 of *Plate*).

It is a small organ, with a rounded top, about 0·3 mm. high and 0·15 mm. broad ; it is composed of small square



Text-fig. 2.

Transverse section of a crushed cone of *Bothrodendron mundum* (Will. A188 D. M. S. Watson's collection from the mountain four foot coal of South Grain Dulesgate. Camera lucida drawing only shading diagrammatic. × 15 approx.

xm = wood.
mc = middle cortex of axis.
oc = outer cortex of axis, displaced.
sph = sporophyll.
lg = ligule in pit in sporophyll.
meg. sm = megasporangium.
mic. sm. = microsporangium.

parenchymatous cells agreeing exactly with those of the ligule of *Miadesmia* and the ordinary Lepidodendraceæ.

The ligule always appears light-coloured in the sections which I have examined. The lamina, which is directed vertically, has a distinct groove on its inner surface which receives the ligule ; this gives an exceedingly characteristic form to transverse sections of the lamina in

its lower part (See *Fig.* 2 of *Plate*). The lamina continues at least to the height of the top of the sporangium, but I could form no satisfactory idea of its structure in this part from existing sections.

Spores.

Each cone carries both micro- and megaspores, and it seems most likely that the microspores were in the uppermost sporangia, the megaspores below, as is the case in Williamson's longitudinal section. In this connection it should be mentioned that the microsporangia are usually smaller than the megasporangia. The microspores have the form of quarter spheres and are about ·027 mm. in diameter.

They are often found associated in tetrads, and except for the ridges produced by this association are without ornament.

The megaspores were several times described by Williamson (Will. '**78**). They are of roughly spheroidal form, and are provided with a projection which interlocks with those of the other spores of the tetrad. Encircling the equator of the spore, assuming the projection to be the pole, is a belt of ramified hairs about 0·15 mm. long. These are exceedingly characteristic, and render the identification of the megaspores easy. No more than four megaspores have been found in any sporangium, and it is almost certain that only one tetrad is normally developed in each sporangium (See *Fig.* 4 of *Plate* and *Text-fig.* 2).

For further illustrations of the megaspores see Williamson '**78**, pl. 23, figs. 58, 59, 60, 64, 66, and '**80**, pl. 15, fig. 9.

Discussion of the Relationship of the Cone to *Bothrodendron* and other Lycopods.

The genus *Bothrodendron* was founded by Lindley and Hutton in their "Fossil Flora" (L. and H. '33). They did not define it satisfactorily, and it was regarded as a condition of preservation of various other genera of Lepidodendraceæ by most palaeobotanists, until Zeiller ('86) shewed that well preserved specimens existed having definite leaf scars, shewing the prints of the vascular bundle and the two parichnos strands.

The genus belongs to the Lepidodendraceæ, and may be roughly described as being a *Lepidodendron* in which the projection of the leaf basis is nil. Nathorst has shewn that *Cyclostigma Kiltorcense* from the upper old red sandstone is really a *Bothrodendron*, the genus also occurs in the lower carboniferous rocks of Scotland, and is hence one of great antiquity.

It has recently been shewn by Mr. Lomax that Williamson's *Lepidodendron mundum* is a *Bothrodendron*, but the surface of the stem on which the identification is founded does not, in my opinion, warrant specific determination. The only fructification yet referred to *Bothrodendron* is a cone described by Kidston ('89), which was attached to a twig of *Bothrodendron minutifolium* Boulay. Owing to the kindness of Mr. Kidston, I have been able to examine in Manchester three examples of this cone, one of which is still attached to an undoubted Bothrodendron twig. These specimens are derived from the Middle Coal Measures of Barnsley, Yorkshire.

I was unable to clearly make out the structure of these impressions, but believe that it was similar to the cone described above, in the fact that the horizontal limb of the sporophyll is short compared to the diameter of

the axis. The horizontal portion of the sporophyll seems to be much more normal, and like that of *Lepidostrobus* in *B. minutifolium* than in *B. mundum.*

Finally, the cones of *B. minutifolium* are long and cylindrical, differing from the tapering cones of *B. mundum.*

From these differences it appears certain that *Bothrodendron mundum* is not *B. minutifolium* Boulay. It appears likely that *B. mundum* must therefore belong to *B. punctatum*, the only other species known as impressions from the English Lower Coal Measures (see note).* To this point I shall return in a paper to be shortly published "On the Ulodendroid scar."

The attribution of the cone described above to *Bothrodendron mundum* (Will.) rests on the following data :—

1. The wood of the axis of the cone corresponds exactly with that of a small twig of *B. mundum.* This correspondence in structure was noticed by Mr. Maslen whilst cataloguing Q454 in the Manchester Museum register.

2. The horizontal portion of the sporophyll between the ligule and the axis corresponding to the leaf base of an ordinary Lepidodendroid stem is much shortened radially when compared with *Lepidostrobus*, a modification homologous to that of the leaf bases of *Bothrodendron.*

3. The two foregoing arguments are supported by constant association of the stems of *Bothrodendron mundum* and the cones or their sporophylls or

*It is possible that other species of *Bothrodendron* may occur in the Lower Coal Measures, but *B. punctatum* and *B. minutifolium* are certainly the only common species.

megaspores, as described above; for example, all the sections, about 20 in number, forming the Williamson series of *Lepidodendron mundum* also contain these characteristic megaspores.

In nearly all the sections containing *Bothrodendron mundum* in the Manchester Museum and my own collections (about 30 in number) I have been able to find these spores.

This series of about 50 sections, shewing the association of the stems and cones, is derived from 3 horizons in the Millstone Grit and Lower Coal Measures (see Stopes and Watson), and the following localities:—Halifax, Huddersfield, Hough Hill, Shore, Bacup, Cloughfoot, and South-Grain collieries at Dulesgate and Laneshaw Bridge, near Colne.

Isolated microsporangiate sporophylls of the *Bothrodendron* cone have been confused with those of *Miadesmia*. This confusion, I believe, arose in the following way: the majority of the specimens of the peculiar "seeds" of *Miadesmia*, first described by Miss Benson in the *New Phytologist*, have occurred in a few blocks from Cloughfoot Colliery, Dulesgate. These blocks contain fragments of at least three distinct types of Lycopodiaceous cones. These cones are the cone described above, the cone of *Miadesmia*, and another small cone which I hope to describe shortly.

The isolated sporophylls of *Miadesmia* and *Bothrodendron* agree in the following particulars. They are both Lycopodiaceous cones in which the sporophylls are not greatly radially extended as in *Lepidostrobus;* they are very small and delicate and nearly of the same size; they are both provided with a large and very conspicuous ligule.

The isolated male sporophylls may be separated by the following characters* :—

1. The *Bothrodendron* sporophylls do not terminate in the characteristic lamina, only one cell thick, of *Miadesmia membranacea*, Bertrand.

2. In *Bothrodendron* there is a great development of transfusion tissue in the sporophyll, particularly round the base of the ligule : in the true *Miadesmia* there is little or none.

3. The vascular bundle in *Bothrodendron* is continued in the lamina up to the level of the top of the sporangium ; in *Miadesmia* it stops sooner.

4. The tip of the ligule is normally rounded in the sporophylls of *Bothrodendron*. I understand from Miss Benson that in *Miadesmia* the tip of the ligule is "lamellar." [Letter, August 24th, 1907.]

Taken as a whole, the cone of *Bothrodendron* differs essentially from that of *Miadesmia* in the following respects :—

In *Bothrodendron* the megasporangium contains four functional megaspores and is not surrounded by an integument ; in *Miadesmia* the sporangium contains only one megaspore and is surrounded by a highly specialised integument.

The idea occurred to me that *Miadesmia* and *Bothrodendron* might be connected as are *Lepidocarpon* and *Lepidostrobus*, but the great differences in stem structure

* I have been unable to compare the sporophylls of *Bothrodendron* with those of *Miadesmia* in much detail owing to the fact that my knowledge of *Miadesmia* is derived entirely from the poor preparations in the Manchester collections. I look forward to the forthcoming monograph, by Miss Benson, on *Miadesmia* to reveal many other points of difference and clear up all the confusion which has arisen.

between *Bothrodendron* and *Miadesmia* seem to disprove any possibility of a connection between them.

Except for the great radial shortness of its sporophylls, the Bothrodendron cone does not differ very much from *Lepidostrobus.*

There is, however, no doubt that it must be regarded as generically distinct from *Lepidostrobus*, in which the radial extension of the sporophylls is a characteristic feature. At the same time there is, I think, no doubt that it is a member of the Lepidodendraceæ. The whole structure of the axis, the occurrence of a bifid parichnos in the stems, and the occurrence of the Ulodendroid conditions shew that it is quite closely related to the "genera" *Lepidodendron*, *Lepidofloios*, and *Sigillaria*, which are the other members of the Lepidodendraceæ as generally accepted. On the whole, the cone seems to differ more from that of any of the three other genera than the latter do amongst themselves.*

It has recently been suggested by Dr. Scott :06 and referred to by Professor Oliver :07 that *Spencerites* may be related to *Bothrodendron.*

The differences between the cone here attributed to *Bothrodendron mundum* and *Spencerites* are as follows:—

1. The attachment of the sporangium to a ventral process at the distal end of the horizontal limb in *Spencerites* is quite different to the simple attachment in *Bothrodendron.*

2. It is improbable that there is a ligule in *Spencerites* (Berridge :05). Since the occurrence of a ligule is regarded by many botanists as an important

* The numerous well-preserved examples of *Sigillariostrobus* which have now been examined leave no doubt that that cone was in all essentials identical with *Lepidostrobus.* I have reasons for believing that certain cones of the *Lepidostrobus oldhamium* type found petrified are Sigillarioid.

taxonomic feature in recent Lycopods, this difference is probably an important one.

3. There is little doubt that *Spencerites* was homosporous (Scott :06). This is an important difference from *Bothrodendron*, which was heterosporous.

These differences are so important that I think it is very improbable that there is any close connection between *Spencerites* and *Bothrodendron* or indeed any other member of the Lepidodendraceæ.

SUMMARY.

A hermaphrodite cone of small size provided with very short sporophylls and a large ligule, but generally conforming to the Lepidostroboid type is shown to agree with *Bothrodendron mundum* (Will.) in the structure of its axis, and in the small radial extension of the leaf bases of its sporophylls. It is shown to occur constantly associated with *Bothrodendron* and is regarded as attributable to it.

Some points of distinction between the sporophylls of *Bothrodendron mundum* and those of *Miadesmia membranacea*, with which it has been confused, are pointed out. The cone is compared with that of *Bothrodendron minutifolium* (Boulay) and with *Lepidostrobus*. It is not regarded as having any close connection with *Spencerites*.

I wish to express my thanks to Professor Weiss for some kind criticism, to Mr. Kidston for very kindly sending for my inspection some specimens of the cone of *Bothrodendron minutifolium* (Boulay) from his own collection, and finally to Miss K. H. Coward for making some drawings for the illustrations.

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Hard bed, Halifax.

Q460. Cash Collection, Manchester Museum.

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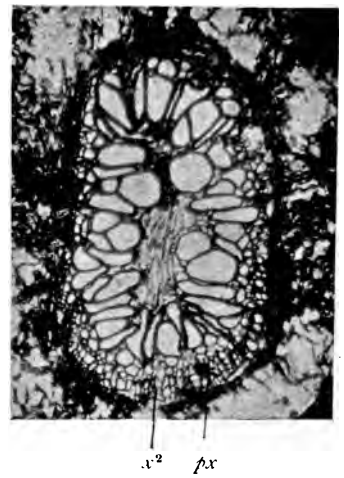


FIG. 3.

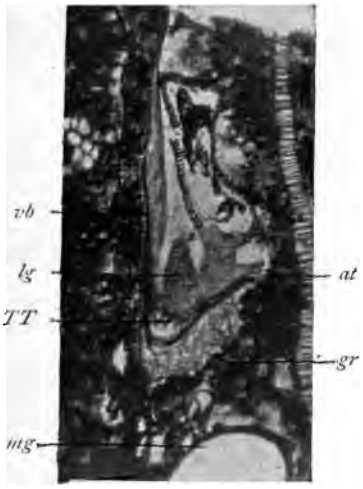


FIG. 4.

Manchester Memoirs, Vol. lii. (1908), *No.* **4.**

IV. "On the Ulodendroid Scar."

By D. M. S. WATSON, B.Sc..

Research Student in the Geological Department of the Victoria University of Manchester.

(Received and Read November 12th, 1907.)

Certain stems of the genera *Sigillaria*, *Lepidodendron* and *Bothrodendron* are found to bear two opposite rows of large depressed scars. Such stems are said to be in the Ulodendroid condition, and were formerly included in the "genus" *Ulodendron.*

These curious Ulodendroid scars were known very early in the study of Palæobotany, and many explanations of their meaning and mode of formation have been put forward.

The explanation which has secured general acceptance is that they bore sessile cones which were not attached to the whole area of the scar, but only to the central or sub-central umbilicus, and which by their pressure, obliterated the leaf bases over the whole area of the scar.

The large size of the scars is explained by the supporters of this theory as due to growth subsequent to formation, and to the shedding of the cones.

Other views which have been put forward, but which have not secured any wide acceptance, are :—

1. That due to Stur, who supposed the scars to have been produced by the pressure of the bases of bulbils.

This view is a modification of the ordinary view, and is explained by the fact that Stur knew that certain

its lower part (See *Fig.* 2 of *Plate*). The lamina continues at least to the height of the top of the sporangium, but I could form no satisfactory idea of its structure in this part from existing sections.

Spores.

Each cone carries both micro- and megaspores, and it seems most likely that the microspores were in the uppermost sporangia, the megaspores below, as is the case in Williamson's longitudinal section. In this connection it should be mentioned that the microsporangia are usually smaller than the megasporangia. The microspores have the form of quarter spheres and are about ·027 mm. in diameter.

They are often found associated in tetrads, and except for the ridges produced by this association are without ornament.

The megaspores were several times described by Williamson (Will. '**78**). They are of roughly spheroidal form, and are provided with a projection which interlocks with those of the other spores of the tetrad. Encircling the equator of the spore, assuming the projection to be the pole, is a belt of ramified hairs about 0·15 mm. long. These are exceedingly characteristic, and render the identification of the megaspores easy. No more than four megaspores have been found in any sporangium, and it is almost certain that only one tetrad is normally developed in each sporangium (See *Fig.* 4 of *Plate* and *Text-fig.* 2).

For further illustrations of the megaspores see Williamson '**78**, pl. 23, figs. 58, 59, 60, 64, 66, and '**80**, pl. 15, fig. 9.

secondary growth. If this be true these small Bothrodendron cones cannot have produced the great Ulodendroid scars of *B. punctatum.*

The largest definite strobilus of a Lepidodendroid tree with which I am acquainted is *Lepidostrobus Brownii*, the internal structure of which is preserved. The diameter of this cone is two inches, and Ulodendroid scars are known about four times this size.

The fact that certain species which bear Ulodendroid scars are known to have cones attached to the ultimate branches suggests that the Ulodendroid scars could not have directly borne cones, as it is exceedingly unlikely that a Lycopod would carry its cones in two distinct ways. There is thus a certain amount of evidence that "*Ulodendron*" could not have had sessile cones attached to its scars.

The principal specimen on which I depend for support to the branch theory is contained in the Manchester Museum. It is represented in *Plates I.* and *II.*

This specimen is a cast in very fine Coal Measure sandstone of one Ulodendroid scar probably referable, on account of its ovoid form and eccentric umbilicus, to *Bothrodendron punctatum* L. & H. The specimen is not localized and not registered, but Professor W. Boyd Dawkins informs me that it is probably from the Middle Coal Measures of Peel Delph Quarry, near Bolton. It is to be particularly noticed that this scar apparently formed the centre of a concretionary area in the sandstone, and that in parts a certain amount of structure is preserved in iron carbonate.

One of the remarkable characters of this specimen is that in the obverse half (see *Figs.* 3, 4, *Plate II.*) the umbilicus is represented by a cylindrical hole 18 mm. deep and 8 mm. in diameter.

This hole is surrounded by an upstanding ring of iron

pyrites and probably iron carbonate, which must represent some portion of the original plant, and which is not a cast.

In the reverse (see *Fig.* 2, *Plate I.*), the continuation of this ring is seen, and it is seen to give origin to numerous small strands which pass outwards. In one place on the lower half of the scar these are intercepted by a plane of fracture, and have the arrangement of dots characteristic of ordinary Ulodendroid scars in this region.

These strands and the tissue in which they run are imperfectly petrified.

I think that the appearances seen on this specimen cannot be explained on the orthodox view, but receive a ready explanation by the theory that the scar represents the base of a branch attached to the whole scar.

The explanation I wish to propose is as follows :—

That a small portion of the base of this branch still remains attached to the scar, and that this patch of tissue was much macerated and decayed before fossilization, all its soft tissues being removed, whilst its vascular system was left almost intact.*

I believe that this fragment of the base of the branch was then imperfectly petrified.

The ring surrounding the depression in the umbilicus is, I believe, the actual primary wood cylinder, and the small strands to which it gives origin are leaf traces which pass outwards to supply leaves attached to the branch.

The size of the Ulodendroid scar is 95 mm. by 80 mm. On the branch theory 80 mm., the horizontal diameter, will represent the diameter of the base of the branch. On the assumption that the upstanding ring round the

*We know from the evidence of petrified stems that this is how Lepidodendroid branches did decay.

umbilicus represents the primary wood, the ratios of the diameter of the wood to that of the branch would be 1 to 8.

Measurements of 9 petrified stems of similar dimensions belonging to *Lepidodendron fuliginosum, L. selaginoides* Car. [*vasculare* Binney], *L. Harcourtii* and *L. Wunschianum* gave 1 to 7·7 as the average ratio of the diameters of wood and stem. I think that these ratios are so close as to lend support to the view I have here adopted.

If the appendicular organ was only attached to the umbilicus then it is very difficult to satisfactorily explain the hole.

It could I think be only explained by supposing that the cortex of the appendicular organs penetrated into the cortex of the stem; it is not easy to see how this could happen to the peduncle of a sessile cone.

The branch theory has the advantage of explaining easily the markings observed on the ordinary Ulodendroid scars.

These markings consist of a central or sub-central point usually represented by a prominence on the specimen.

This umbilicus is surrounded by imprints or projections covering the surface of the scar; these projections are arranged so as to form a series of helices starting from the umbilicus and passing outward to the edge of the scar, forming, as Hugh Miller said, an engine-turning pattern.

They are, in fact, on at least the lower portion of the scar, arranged exactly as the leaf traces are in a transverse section of a Lepidodendroid stem.

They have already been figured by Carruthers, Kidston, and many other palæobotanists. In circular

scars with a central umbilicus these dots are usually continued all round the scar, but in oval scars with an eccentric umbilicus they only cover the lower portion, the upper being covered by longitudinal markings radially arranged, which, however, gradually shorten at the sides of the scar and shade off into the dots of the lower part. I do not see how this arrangement, which is the normal one, can be satisfactorily accounted for on the theory that the dots represent crushed leaf-bases, which is that adopted by the upholders of the orthodox cone view.

If they do represent crushed leaf-bases then these must vary very greatly in size and shape over the area of the scar, which however is supposed to have been only an ordinary piece of the stem surface.

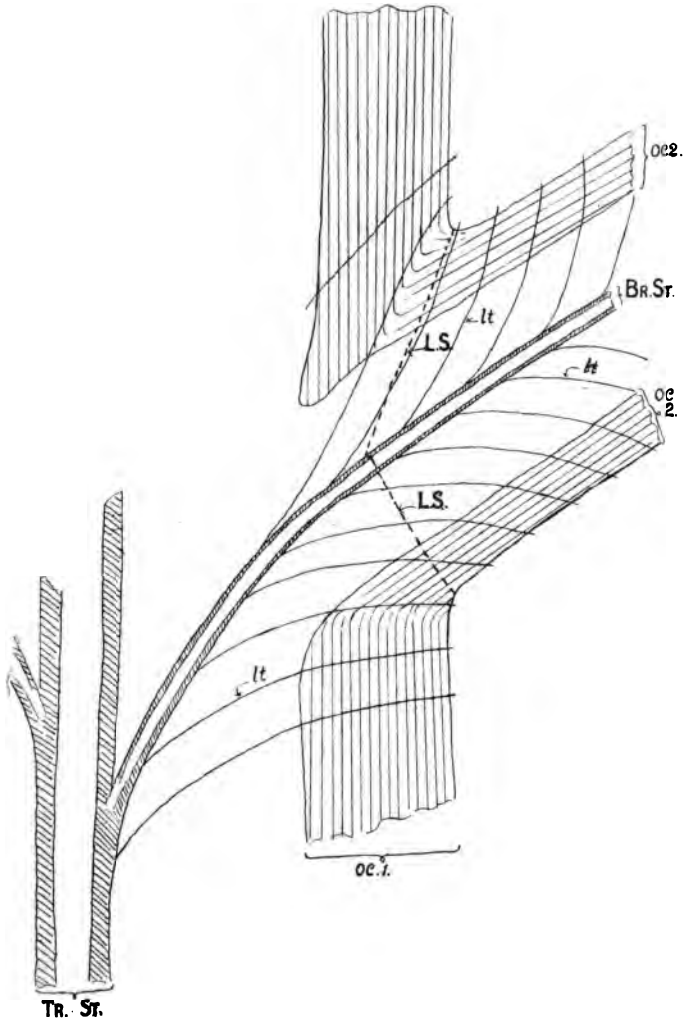
On the branch theory the dots merely represent the cut ends of vascular bundles supplying leaves formerly attached to the branch, and the difficulty of their varying distances apart does not arise; on this theory also the longitudinal markings on the upper part of the scar are accounted for as being obliquely and sometimes longitudinally cut leaf-traces.

It must be pointed out that in the specimen described above, which shows the leaf-traces and their origin from the wood, these explanations seem to work perfectly.

The *Text-figure* will, I hope, make the meaning of this explanation more clear; it should be compared with a somewhat similar figure given by Carruthers.

The branch theory also explains the fact that Ulodendroid scars are often provided with a raised rim at the edge. It simply represents, I take it, a scrap of the outer cortex of the branch left connected to the parent stem.

An additional argument in favour of the branch view, is that we know that some Lepidodendra, of which the structure is known, did bear their branches in two



Diagrammatic section through the base of a branch to illustrate the Branch theory of the Ulodendroid Scar.

oc.1. = Outer cortex [periderm] of main stem.
oc.2. = ,, ,, of branch.
Tr. St. = wood cylinder of main stem.
Br. St. = ,, of branch.
lt = leaf-trace.
L.S. = line of separation of branch to leave in Ulodendroid scar.
Wood shaded diagonally.
Periderm shaded longitudinally.

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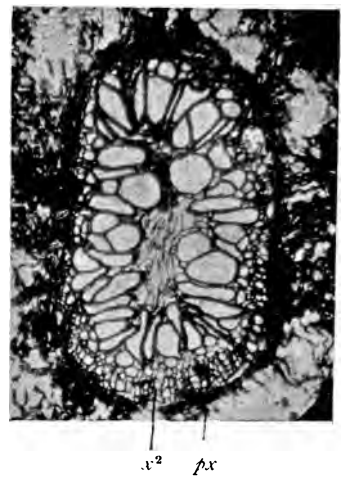

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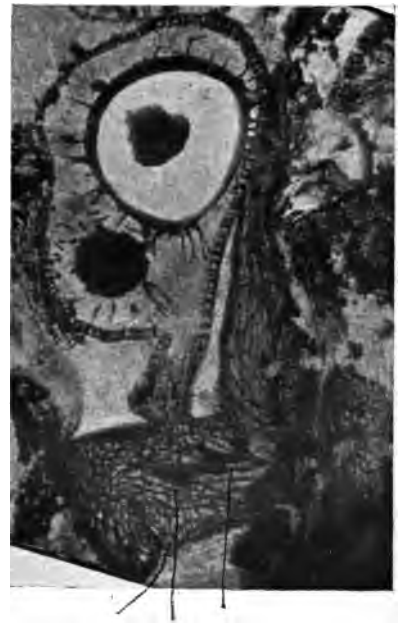

Manchester Memoirs, Vol. lii. (1908), *No.* 4.

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species which occur in the Ulodendroid condition bear their cones attached to the fine ultimate twigs.

It seemed to him very unlikely that a Lycopod should bear cones in two distinct ways, so he was forced to discover some other organ to take the place of a cone, and, remembering the bulbils of *Lycopodium selago*, applied the idea to *Lepidodendron*.

2. Another idea, due to Carruthers, is that the scars represent the bases of roots which arose adventitiously on the stems and were attached to the whole area of the scar.

This view has been combatted by Williamson and Kidston.

The theory that I intend to support in this paper is that the scars are those of branches which were attached to the whole area of the scar.

In a recent paper (Watson :07) I described the cone of *Bothrodendron mundum* Will. and shewed that it did not belong to *B. minutifolium*, Boulay, but probably to *B. punctatum*, L. & H.

This cone is extremely small, less than a centimetre in diameter, and quite short, and it is very difficult to see how it could have produced the enormous Ulodendroid scars which often occur in *B. punctatum*. If it be suggested that the Ulodendroid scars grew after formation more difficulties are raised, for, whilst it is certain that Lepidodendroid stems increased in diameter by secondary growth, it is very unlikely that they would elongate vertically after becoming sufficiently adult to bear cones. If these scars did grow appreciably I think that the tendency of such growth would be to produce scars broader than high, and I think no examples of such scars are known. This fact alone leads one to suppose that the organs producing the scars must have been attached to the stem until the cessation by death or otherwise of

secondary growth. If this be true these small Bothrodendron cones cannot have produced the great Ulodendroid scars of *B. punctatum.*

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Fig. 3. The Ulodendroid scar itself viewed normally to the stem. × about $\frac{1}{2}$.

The opposite half to *Fig.* 1.

X = the upstanding ring round the hole in the Umbilicus.

Lt = Leaf-trace cut transversely shewing with the other traces the Engine-turning pattern.

Fig. 4. The subject of *Fig.* 3 viewed from above and in front, looking directly down the hole. × about $\frac{1}{2}$.

H = the hole in the Umbilicus, 18mm. deep, surrounded by the upstanding ring representing the wood.

(Owing to the lighting employed this hole does not appear so deep as it really is).

Fig. 3.

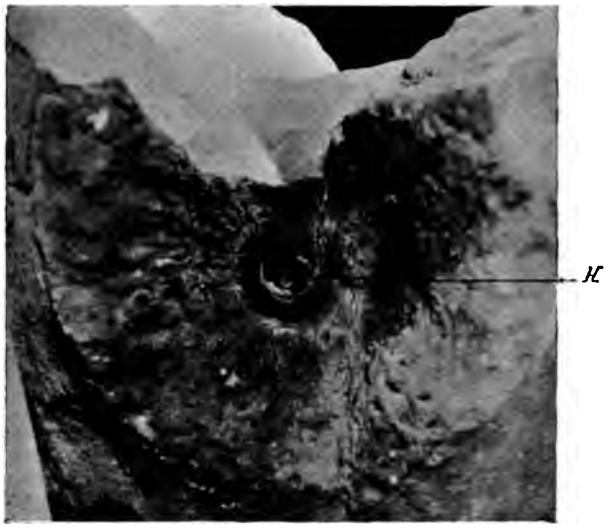


Fig. 4.

Manchester Memoirs, Vol. lii. (1908), *No.* 5.

V. On a new Phytophagous Mite, *Lohmannia insignis*, Berl. var. *dissimilis* n. var., with notes on other species of economic importance.

By C. GORDON HEWITT, M.Sc.,
Lecturer in Economic Zoology, University of Manchester.

Received and Read December 10th, 1907.

The species of mites to which these notes refer have been sent to me for identification during the past two years, and as one of them is a new and interesting variety, and the other two species are of some economic importance, these notes have been written.

Lohmannia insignis, Berlese var. *dissimilis*, n. var.

The *Oribatidae*, to which family of the Acarina this mite belongs are phytophagous, and in the adult stage usually possess tracheae. The anterior portion of the body, the cephalothorax, is usually divided from the abdomen by a transverse constriction. On the dorsal side of the cephalothorax a pair of pseudostigmatic organs are borne by a pair of stigmatic-like tubes, the pseudostigmata.

The species, *Lohmannia insignis*, of which this is a new and distinct variety, was recorded by Berlese in 1904; the specimens were sent to him by Colonel F. N. Halbert from Ireland.

I obtained my specimens from some tulip bulbs which Professor S. J. Hickson brought to me from his garden in Withington early in May of 1907. The tulips produced leaves, and gradually died off without flowering. On examining the bulbs I discovered considerable numbers of the mite beneath the scaly epidermis of the

scale-leaves. A number of the bulbs were kept, and when they were examined six months later not a single specimen of this species could be found, but I found large numbers of *Rhizoglyphus echinopus* to which I shall refer later. Mr. Michael, to whom I submitted some specimens, pointed out to me its similarity to *L. insignis*, Berl., remarking that the length of the hairs differed. Professor Berlese very kindly sent me his type specimen of that species to examine and compare. As a result of this comparison I found a number of distinct and striking differences between *L. insignis* and my specimens, but although these differences might be considered of specific value by those acarologists who, unfortunately, make new species upon very small differences, I think it is preferable to regard the mite as a variety of *L. insignis*.

This variety (*Pl. figs.* 1 & 2) resembles *L. insignis* in several major characters. It is similar in colour, being a dull brown with darker legs. It is also similar in size, its average length being 1mm. The abdomen is also very similar in shape. The differences however, are well marked. The cephalothorax is not pointed as in *L. insignis*, but is more rounded at the rostral extremity; in this character it is somewhat intermediate between *L. insignis* and *L. cylindrica* Berl. The arrangement and number of hairs on the cephalothorax is quite different. In *L. insignis* var. *dissimilis* there are two fairly long median hairs, and a very small pair, slightly more posterior. There is a pair of long inter-stigmatic hairs; anterior to these there is a pair of shorter hairs. In the anterior lateral region of the notogaster, there are three pairs of hairs which are absent in *L. insignis*. This variety has also a few other additional abdominal hairs, but there are fewer hairs on the legs. These differences are constant, so that they do not constitute a sexual difference. On

account of the presence of the median cephalothoracic hairs which is a very exceptional character, I have called this variety *Lohmannia insignis* var. *dissimilis.*

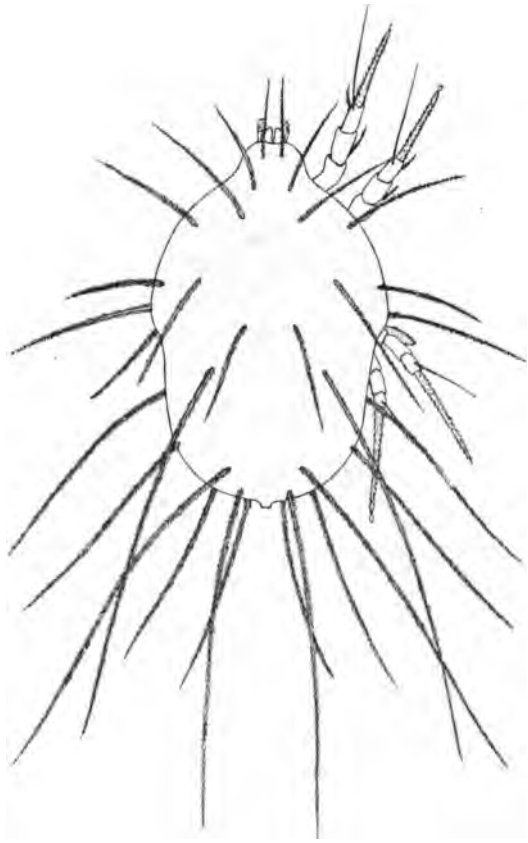
Glycyphagus spinipes Koch.

This species of mite and *Rhizoglyphus echinopus* are members of the family Tyroglyphidae, of which the typical and best known example is *Tyroglyphus siro*, the cheese mite. The group is one of considerable economic importance on account of the damage for which many of the members are responsible and the annoyance caused by others on account of their enormous numbers.

Glycyphagus spinipes (*Text-fig.* A) is similar in its habits to its near relative *G. domesticus* de Geer, with which it is often confused, especially as it has at first sight a structural resemblance to that species. On careful examination several important differences will be found. The body of *G. spinipes* is constricted anterior to the third pair of legs and is narrower behind this region, in *G. domesticus* there is no such constriction. The slender tarsi of *G. spinipes* are longer than those of *G. domesticus* and are covered with fine short hairs, hence the specific name of this mite. *G. spinipes* also bears a small bract-like scale on the third joint of the third leg (see *Fig.*)

This species is pearly white in colour; the males measure about ·5mm. and the females ·7mm. in length. The body is covered with a large number of long and finely pectinated hairs; the number and arrangement of these can be seen from the figure, the longest hairs are those on the posterior half of the body. These mites are able to run with considerable rapidity, and when they occur they swarm in such substances as dried animal and vegetable matter. I have received specimens in the

horse-hair stuffing of chairs, where they appeared to be feeding on the greasy substances in the hair. Oudemans (1897) also records its occurrence in the same material in Holland.



Text-fig. A. *Glycyphagus spinipes.* ♀. × 70. Dorsal aspect.

The worst case of infection of this species which I have known was in a house in the neighbourhood of Manchester, in 1905-6. The house had been recently built, and the furniture was new. Shortly after the house had been occupied this mite was found in practically all

the rooms. By the use of considerable quantities of naphthaline, and also ammonia, carbolic acid, and turpentine, etc., it was eradicated from all the rooms except one, to exterminate it from which all efforts failed. Each morning the furniture of this room would be covered over with a fine, white dust, which consisted of thousands of mites ; the room could not be used on account of the presence of the mite. It was at this juncture that I was asked for advice as to the pest and its eradication. Discovering that it was *G. spinipes*, I came to the conclusion that it was in the stuffing of the furniture of the room, which probably had served as the original source of infection and distribution. I recommended the owners to send the furniture and carpet away to be stoved, after which the room was to be made as airtight as possible, and fumigated several times with nicotine vapour at intervals of a week or ten days ; after this procedure, the paper was to be stripped from the walls, and the walls and floor thoroughly washed with a fairly strong solution of carbolic acid, and afterwards it was to be repainted and papered. These recommendations were carried out early in 1906. In August, 1907, I was informed on inquiry that the mite had not returned, and that the house was entirely free from the pest. To be certain of eradicating the pest, the owners had disposed of the suite of furniture after it had been stoved, as I had suggested that it was the original source of infection.

This case and others which have occurred point to the necessity of having the materials, such as the stuffing of chairs and mattresses, which are used in the preparation of the adjuncts of a comfortable age, properly treated so as to render them unattractive by the absence of food matter to such annoying pests as these species of Tyroglyphidae, which are not injurious to man, but become a

Fig. 3. The Ulodendroid scar itself viewed normally to the stem. × about $\frac{1}{2}$.

The opposite half to *Fig.* 1.

X = the upstanding ring round the hole in the Umbilicus.

Lt = Leaf-trace cut transversely shewing with the other traces the Engine-turning pattern.

Fig. 4. The subject of *Fig.* 3 viewed from above and in front, looking directly down the hole. × about $\frac{1}{2}$.

H = the hole in the Umbilicus, 18mm. deep, surrounded by the upstanding ring representing the wood.

(Owing to the lighting employed this hole does not appear so deep as it really is).

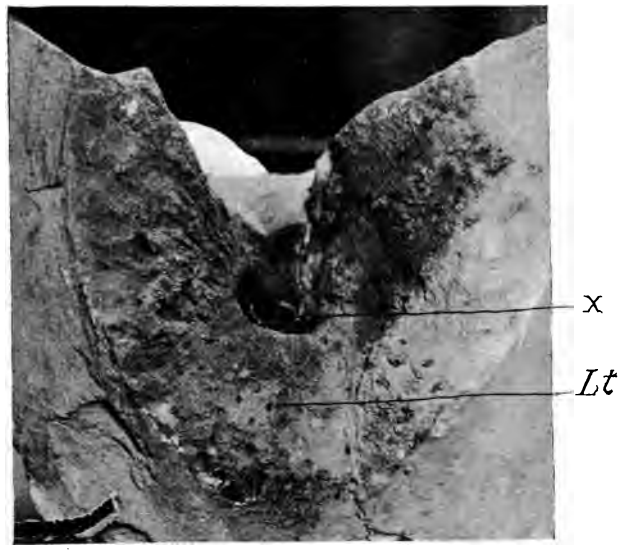


Fig. 3.

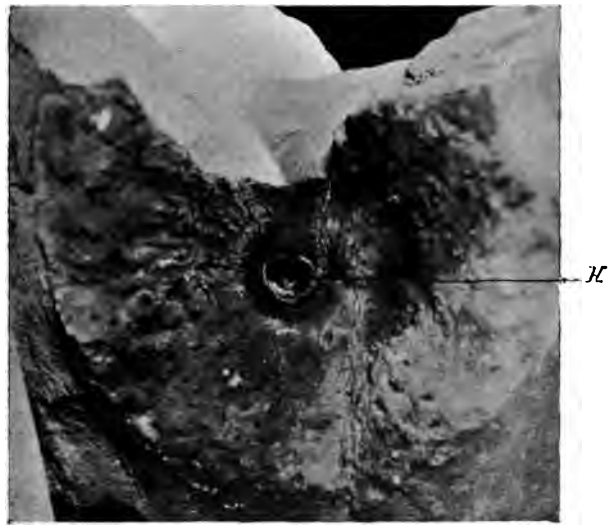


Fig. 4.

Manchester Memoirs, Vol. lii. (1908), *No.* **5.**

V. On a new Phytophagous Mite, *Lohmannia insignis*, Berl. var. *dissimilis* n. var., with notes on other species of economic importance.

By C. GORDON HEWITT, M.Sc.,
Lecturer in Economic Zoology, University of Manchester.

Received and Read December 10th, 1907.

The species of mites to which these notes refer have been sent to me for identification during the past two years, and as one of them is a new and interesting variety, and the other two species are of some economic importance, these notes have been written.

Lohmannia insignis, Berlese var. *dissimilis*, n. var.

The *Oribatidae*, to which family of the Acarina this mite belongs are phytophagous, and in the adult stage usually possess tracheae. The anterior portion of the body, the cephalothorax, is usually divided from the abdomen by a transverse constriction. On the dorsal side of the cephalothorax a pair of pseudostigmatic organs are borne by a pair of stigmatic-like tubes, the pseudostigmata.

The species, *Lohmannia insignis*, of which this is a new and distinct variety, was recorded by Berlese in 1904; the specimens were sent to him by Colonel F. N. Halbert from Ireland.

I obtained my specimens from some tulip bulbs which Professor S. J. Hickson brought to me from his garden in Withington early in May of 1907. The tulips produced leaves, and gradually died off without flowering. On examining the bulbs I discovered considerable numbers of the mite beneath the scaly epidermis of the

scale-leaves. A number of the bulbs were kept, and when they were examined six months later not a single specimen of this species could be found, but I found large numbers of *Rhizoglyphus echinopus* to which I shall refer later. Mr. Michael, to whom I submitted some specimens, pointed out to me its similarity to *L. insignis*, Berl., remarking that the length of the hairs differed. Professor Berlese very kindly sent me his type specimen of that species to examine and compare. As a result of this comparison I found a number of distinct and striking differences between *L. insignis* and my specimens, but although these differences might be considered of specific value by those acarologists who, unfortunately, make new species upon very small differences, I think it is preferable to regard the mite as a variety of *L. insignis.*

This variety (*Pl. figs.* 1 & 2) resembles *L. insignis* in several major characters. It is similar in colour, being a dull brown with darker legs. It is also similar in size, its average length being 1mm. The abdomen is also very similar in shape. The differences however, are well marked. The cephalothorax is not pointed as in *L. insignis*, but is more rounded at the rostral extremity; in this character it is somewhat intermediate between *L. insignis* and *L. cylindrica* Berl. The arrangement and number of hairs on the cephalothorax is quite different. In *L. insignis* var. *dissimilis* there are two fairly long median hairs, and a very small pair, slightly more posterior. There is a pair of long inter-stigmatic hairs; anterior to these there is a pair of shorter hairs. In the anterior lateral region of the notogaster, there are three pairs of hairs which are absent in *L. insignis.* This variety has also a few other additional abdominal hairs, but there are fewer hairs on the legs. These differences are constant, so that they do not constitute a sexual difference. On

account of the presence of the median cephalothoracic hairs which is a very exceptional character, I have called this variety *Lohmannia insignis* var. *dissimilis.*

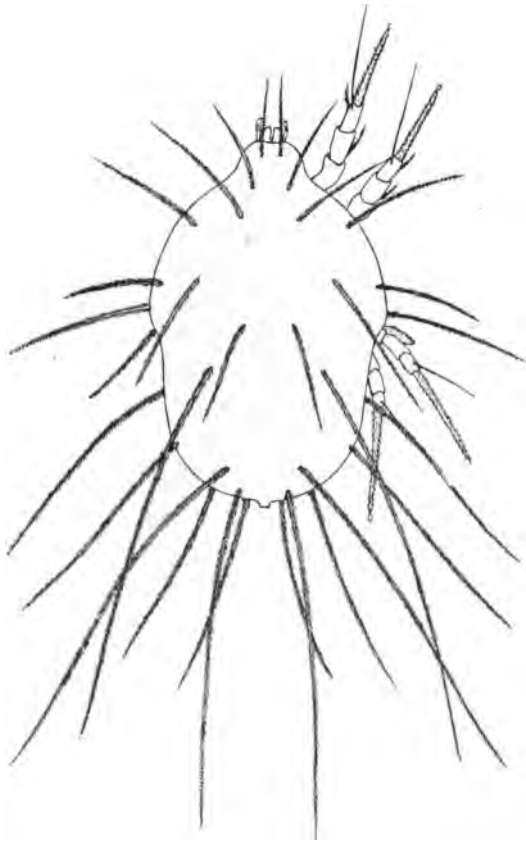
Glycyphagus spinipes Koch.

This species of mite and *Rhizoglyphus echinopus* are members of the family Tyroglyphidae, of which the typical and best known example is *Tyroglyphus siro*, the cheese mite. The group is one of considerable economic importance on account of the damage for which many of the members are responsible and the annoyance caused by others on account of their enormous numbers.

Glycyphagus spinipes (*Text-fig.* A) is similar in its habits to its near relative *G. domesticus* de Geer, with which it is often confused, especially as it has at first sight a structural resemblance to that species. On careful examination several important differences will be found. The body of *G. spinipes* is constricted anterior to the third pair of legs and is narrower behind this region, in *G. domesticus* there is no such constriction. The slender tarsi of *G. spinipes* are longer than those of *G. domesticus* and are covered with fine short hairs, hence the specific name of this mite. *G. spinipes* also bears a small bract-like scale on the third joint of the third leg (see *Fig.*)

This species is pearly white in colour; the males measure about ·5mm. and the females ·7mm. in length. The body is covered with a large number of long and finely pectinated hairs; the number and arrangement of these can be seen from the figure, the longest hairs are those on the posterior half of the body. These mites are able to run with considerable rapidity, and when they occur they swarm in such substances as dried animal and vegetable matter. I have received specimens in the

horse-hair stuffing of chairs, where they appeared to be feeding on the greasy substances in the hair. Oudemans (1897) also records its occurrence in the same material in Holland.



Text-fig. A. *Glycyphagus spinipes.* ♀. × 70. Dorsal aspect.

The worst case of infection of this species which I have known was in a house in the neighbourhood of Manchester, in 1905-6. The house had been recently built, and the furniture was new. Shortly after the house had been occupied this mite was found in practically all

the rooms. By the use of considerable quantities of naphthaline, and also ammonia, carbolic acid, and turpentine, etc., it was eradicated from all the rooms except one, to exterminate it from which all efforts failed. Each morning the furniture of this room would be covered over with a fine, white dust, which consisted of thousands of mites ; the room could not be used on account of the presence of the mite. It was at this juncture that I was asked for advice as to the pest and its eradication. Discovering that it was *G. spinipes*, I came to the conclusion that it was in the stuffing of the furniture of the room, which probably had served as the original source of infection and distribution. I recommended the owners to send the furniture and carpet away to be stoved, after which the room was to be made as airtight as possible, and fumigated several times with nicotine vapour at intervals of a week or ten days ; after this procedure, the paper was to be stripped from the walls, and the walls and floor thoroughly washed with a fairly strong solution of carbolic acid, and afterwards it was to be repainted and papered. These recommendations were carried out early in 1906. In August, 1907, I was informed on inquiry that the mite had not returned, and that the house was entirely free from the pest. To be certain of eradicating the pest, the owners had disposed of the suite of furniture after it had been stoved, as I had suggested that it was the original source of infection.

This case and others which have occurred point to the necessity of having the materials, such as the stuffing of chairs and mattresses, which are used in the preparation of the adjuncts of a comfortable age, properly treated so as to render them unattractive by the absence of food matter to such annoying pests as these species of Tyroglyphidae, which are not injurious to man, but become a

pest on account of their enormous numbers and rapid multiplication.

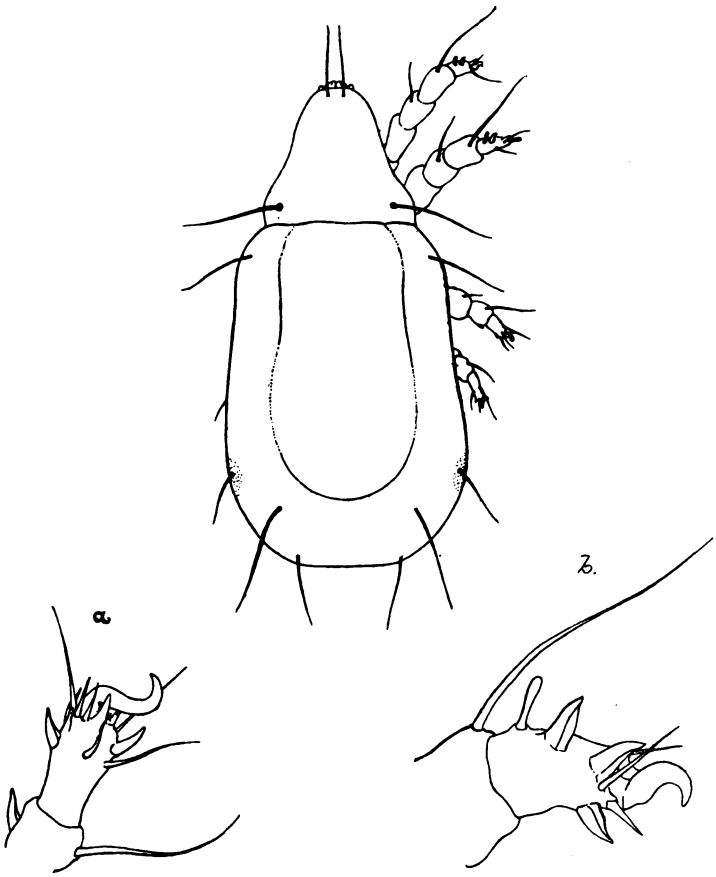
Rhizoglyphus echinopus, Fumouze & Robin (1868).

This bulbicolous mite has been described under a number of different names, as will be seen from the bibliography in Michael's description of the species (1903): Murray in his handbook (1877) describes it under three names. This multiplicity of specific names is due, I think, to the large amount of variation which is found in different specimens, especially males, and also their different food habits. Banks (1906) in his recent memoir on the American Tyroglyphidae calls this species *R. hyacynthi* Boisduval. As Michael has already pointed out (*t.c.*), this name cannot be satisfactorily followed, as the habitat is the only clue which we have to its being this species, on this account I think it is preferable to retain the specific name *echinopus* of Fumouze & Robin. Banks also retains Riley's species *R. phylloxerae* distinct, although Michael considers the species to be *R. echinopus*.

I do not think in view of the variations which occur in this and other species of mites, that Banks is quite justified in his multiplication of the species.

The average length of mature specimens is a little over ·5mm., the males being slightly smaller than the females. They are yellowish-white in colour, the legs being reddish-brown. The anterior region of the body, the cephalothorax, is conical and is divided from the abdomen by a distinct groove. The sides of the abdomen bear a number of setae, the arrangement and number of these in the male is shown in the figure. The female has a larger number of setæ, an additional pair is situated in front of the antero-lateral abdominal setæ, and several

additional pairs are found at the posterior end of the abdomen. These setæ, in the females especially, are subject to considerable variations in size. The specific



Text-fig. B. *Rhizoglyphus echinopus.* ♂. × 125. Dorsal aspect.
a. Terminal joint (tarsus) of third left leg.
b. Terminal joint of first right leg.

name *echinopus* refers to the nature of the legs of the species which bear a number of spines as shown in *Text-*

fig. B *a* and *b*. Michael is doubtful whether the number of spines is constant and Canestrini makes the number different; I have examined a number of specimens and find that they are fairly, though not absolutely, constant in number and position. The claws are stout and strongly curved. The genital opening is situated between the bases of the posterior pair of legs and the anus is more posterior on the ventral side.

This species was found on tulips, together with *L. insignis* var. *dissimilis*. The hyacinth and tulip are the chief food plants of this mite; bulbs of liliaceous plants appear to be chiefly attacked, and it is popularly known as the "bulb mite." It is also called the "*Eucharis* mite," on account of its apparent preference for the bulbs of that plant, but it is no doubt more frequently found on *Eucharis* bulbs, on account of their greater value, which results in greater care and examination of the bulbs. Claparède (1868) found it in hyacinth bulbs, and also on dahlia and potato tubers; in the one case he named the species *R. dujardini*, and in the other he gave it the name *R. robini*, not recognising that the male was dimorphic in the character of the third pair of legs, which in some specimens are very stout; he mistook the thick-legged form of the male for the female. It has been recorded on the roots of the vine. As it is responsible for considerable damage to such plants as hyacinths, tulips, onions, etc., this mite is of economic importance. It is supposed by some authors to follow decay, but I have found it attacking fresh bulbs, and Michael finds that it usually prefers these. I have not only found it between the scales, but also beneath the epidermis, where it was attacking the softer tissues. Apart from the damage for which it is directly responsible, it provides entrance for fungal and bacterial organisms, which produce further decay.

As in the case of the majority of mites, it is extremely difficult to eradicate when it has established itself in the garden, as it lives beneath the surface of the soil. The safest remedy is to burn the infected bulbs and roast the adjacent earth. If the bulbs are considered to be too valuable to burn, they might be carefully treated with paraffin or kerosine several times at intervals of a week or two. Fumigating in a small chamber with nicotine or hydrocyanic gas at intervals, as the gas will not kill them in the egg state, would probably destroy the mites.

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EXPLANATION OF PLATE.

Lohmannia insignis, Berl. var. ***dissimilis***, n. var.

Fig. 1. Dorsal aspect. × 75.

Fig. 2. Ventral aspect. × 48.

Fig. 1.

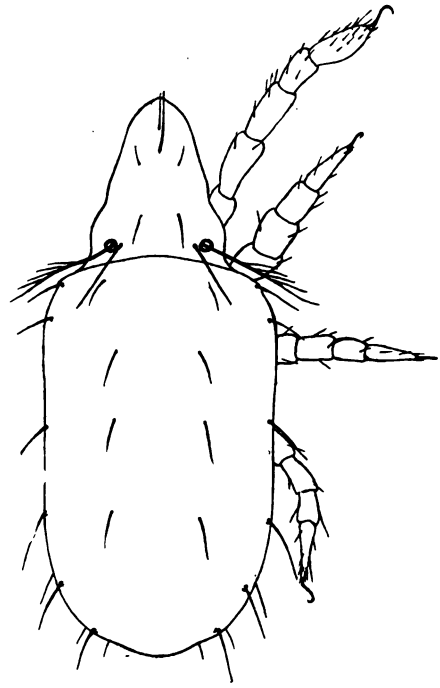

Fig. 2.

C.G.H. del.
O.V.D. photo.

Manchester Memoirs, Vol. lii. (1908), *No.* **6**.

VI. Some Notes on the Mammals of Lundy.

By T. A. COWARD, F.Z.S.

Received and read, December 10th, 1907.

The Mammalian fauna of Lundy has not received the same attention as some other branches of its fauna and flora, for instance as its birds, beetles, and land and fresh-water shells. All British mammals have until recently been much neglected, and the number of species found on Lundy is so small that the collector has not been attracted. Incidentally its mammals have been referred to in journals and newspapers, but there is no complete list; the statements made by J. R. Chanter in his "Lundy Island" (1), published in 1877, are frequently quoted as if they comprised all that could be said on the subject. To correct a few misconceptions and add a little to the knowledge of the Island I offer these notes, the result of a few days spent on Lundy.

Mr. Charles Oldham and I were on Lundy from August 28th to September 4th, 1907, and put down traps every night in likely spots for Rats, Mice, Voles, and Shrews, but the majority of our traps were empty in the morning or had been sprung by slugs. Slugs, indeed, were very troublesome; attracted by the bait they crawled over the traps, releasing the springs and frequently being captured, possibly early in the evening before small mammals had begun to feed. We thus secured few specimens of the animals we wanted, though the runs of mammals were abundant in some places; winter trapping would probably be more productive.

January 22nd, 1908.

Lundy Island, lying about 12 miles N.N.W. of Hartland Point on the North Devon coast and over 30 miles south of the coast of Pembroke, is a granite and slate tableland of about 3½ miles from north to south and less than a mile from east to west at the widest point. It rises to about 450 to 500 feet above the sea and is surrounded by steep and weathered cliffs, except at the southern end where there are two beaches—on either side of a narrow neck of land—that on the east being the only one available for landing purposes. From this beach a steep lane ascends a sheltered combe, where there are a few small trees and much thick undergrowth, cultivated ground and the walled and terraced gardens round the residence of the owner, the Rev. H. G. Heaven. We set most of our traps in the bracken and bramble-covered banks of this lane and, by permission, in Mr. Heaven's kitchen-gardens. The vegetation on the eastern and more sheltered slopes is much more luxuriant than on the west; the old turf banks are thickly tangled with bramble, fern, honeysuckle, and other plants, and in small combes bracken and osmunda grow to a great height; ling and heather is thicker and finer on the eastern than on the more exposed western cliffs. The trees—mostly ash, sycamore, willow and mountain ash—have been planted within recent years, and are small and insignificant. Camden (2) says—"Trees it hath none but stinking Elders," and T. V. Wollaston, so recently as 1845 (3), declared that it "has not so much as a *single* tree to boast of." This is not the case to-day; though there are no large trees, the combe referred to, has been well planted, and the trees appear to be healthy.

At the southern extremity is a slate islet, cut off from the mainland at high tides only, which shelters the bay from the south; it is called Rat Island, and is covered

with a thick cushion of coarse grass through and beneath which are numerous Rat runs and burrows. The earliest reference to this islet under the name Rat Island, which I have been able to find, is in a manuscript journal of a friend of Benson's, one time lessee of Lundy, and a notorious character in connection with its dark history. I have not seen the original, but an extract is quoted by Chanter which indicates that the name was in use in 1752.

Various walls, ruins of cottages and enclosures, show that at different times much of Lundy has been under cultivation, but at the present day the farms, cottages, and cultivated ground are all at the southern end; the greater part of the island is rough moorland, where a few cattle are grazed.

"The indigenous terrestrial mammalia," says Chanter, "are represented by two animals only—rats and rabbits, both of which abound. The old English black rat, *mus rattus*, is the indigenous and until recently was the only species on the Island; but of late years the Norway, or brown rat, has found his way there, most probably from some shipwrecked vessel, and bids fair to exterminate the native breed." On the next page he contradicts his previous statement by adding a third species. "The shrew mouse is also found; but no other mice, nor moles, stoat, or other vermin, nor any snakes or reptiles, exist on the Island." Mr. Heaven, however, informs us that Bats occur, but as we did not see any we can give no suggestion as to the species.

The "shrew mouse" of Lundy is probably the Lesser Shrew, *Sorex minutus*, Linn. According to report, a Shrew is common; in the banks of the lane, as well as at one or two places on the eastern slopes above the cliffs, the runs of some small mammal were very abundant. We only succeeded in capturing one Shrew, and this was

a Lesser—a large adult female, measuring in head and body 55mm., and tail 41mm.; but I have particulars of two other shrews from Lundy.

Mr. A. J. R. Roberts, in 1887, obtained one, which he thought was a Common Shrew, *Sorex araneus*, Linn., but which he says measured from tip of snout to root of tail from 1½ to 2 inches; this specimen was never examined by a competent authority, but it was, judging by its size, either a Lesser or an immature Common Shrew. More recently Mr. Norman H. Joy sent a Lundy Shrew to the British Museum where it was examined and identified as *Sorex minutus*. We thus know that the Lesser Shrew does occur but have no satisfactory evidence that the Common Shrew is found on Lundy.

If it was true in 1877 that no Mice occur on Lundy it is not true to-day; the House Mouse, *Mus musculus*, Linn., has found its way there and is an inhabitant of the houses. We captured one in a disused farm enclosure. Mr. Joy, in a letter to the British Museum, which I have seen, says "There is also a yellow Mouse which is taken under hay," but he spoke from report only and had not seen an example. He suggests the Wood Mouse or Harvest Mouse. It is possible that *Mus sylvaticus*, Linn., does occur, but it is remarkable, if this is so, that we did not capture any in our traps, for the Wood Mouse is one of the easiest mammals to trap. We found neither the Field nor Bank Vole.

The Brown Rat, *Mus norvegicus*, Erxl., has not apparently succeeded in exterminating the Black Rat, *Mus rattus*, Linn., for both occur on the island. It has been suggested to me that the Black Rat may have been exterminated more than once and re-introduced from some of the many wrecks. Before the Trinity House alterations, when the one lighthouse standing in a high

and central position was re-placed by lower and more powerful lights at the north and south of Lundy, wrecks were of frequent occurrence, and the majority of the Black Rats which occur in the British islands to-day are found in seaports, where they have, without doubt, been introduced by ships. While admitting the possibility of frequent additions to the Brown and Black Rat population of Lundy by refugees from wrecks, there are so many references to the Black Rats of Lundy, since the date when the Brown Rats were supposed to have first appeared, that I do not think it probable that the later introduction has ever succeeded in entirely driving out the other.

Chanter quotes from Camden (2) that "the whole land swarms with rabbits and black rats," but in the 1594 Latin and 1637 English editions, which I have consulted, there is no mention of Rats; I fancy from the modernised wording of Chanter's quotation that it must be taken from a more recent edition, which some editor has tampered with.

The earliest reference to Rats which I have found, is in the same MS. journal which first mentions Rat Island, referring to affairs in 1752. "Had it not been for the supply of rabbits and young sea-gulls our table would have been but poorly furnished, rats being so plenty that they destroyed every night, what was left of our repast by day." This quotation is extracted from Chanter's "Lundy Island" and not from the original, and from the same source I have taken the words of the Rev. Thomas Martyn of Cambridge, who gave evidence about the island in Chancery proceedings in 1776. "It was so overrun with rats and rabbits, that any crops which might be produced thereoff, would, as he apprehended, be infallibly devoured by them."

Francis Grose, who according to Chanter, described the island in 1775, though the reference is in vol. VI., which was published in 1785 (4), is the first to refer to the Black Rat specifically. "Rats are so numerous here as to be very troublesome; they are all of the black sort; the great brown rat which has extirpated this kind all over England, not having yet found its way into the island of Lundy."

Chanter further tells us, referring to the arrival of the Brown Rat, that "Mr. Heaven, writing this present year" (either 1871 or 1877), "reports them as increasingly numerous, and the black rat nearly extinct. They principally frequent the south end, and Rat Island swarms with them. They are believed to feed largely on fish, as well as on limpets and other littoral prey. Specimens of a third variety, of a reddish or fox-colour, are sometimes seen and killed. It is called locally the red rat. It has much larger ears, and a longer and thinner tail than the ordinary rat, but in other respects resembles it, and they appear to consort together..... It is scarce, and is but rarely captured, but is persistent on the Island."

Mr. Wollaston (3) mentions the Rats, which he says "grow to an unusually large size, and, not content with a mere *theoretical* existence, are amongst the first to make your acquaintance on landing, more particularly if you come, as is perfectly necessary, well laden with provisions."

At night we frequently heard Rats in the Manor House garden, where we captured two young Brown Rats. We secured eight Black Rats, seven of them in or just outside Mr. Heaven's kitchen gardens, where they had practically ruined his crop of peas and had been feeding on fallen apples and other produce.

Mr. J. G. Millais (5) divides *Mus rattus*, Linn., into three sub-species, geographical races which, when intro-

duced artificially into any locality may interbreed and produce many intermediate forms. Three* out of the eight were more or less typical *Mus rattus rattus*—the *Mus rattus* of Linnaeus, blue-black above and slate-grey beneath, and five* were the brown form, *Mus rattus alexandrinus*, generally known as the Alexandrine Rat, the *Mus alexandrinus* of Geoffroy—brown on the upper parts and with white bellies. This Alexandrine Rat, though the examples captured could hardly be called red or foxy, is probably the mysterious Red Rat. This species, however, is very variable, and examples from the Mediterranean basin are usually ruddy : introductions by ships from the Mediterranean might easily be called red.

We set traps on Rat Island on one day only, but though the majority were sprung we only captured a single example, a large male *Mus rattus alexandrinus*. The general colour of the upper surface of the five examples of *Mus rattus alexandrinus*, four of which are females, varies from "wood-brown "** to "broccoli-brown" on the flanks, but the brown hair is so interspersed with glossy grey, black-tipped hairs that the whole upper surface has almost a "sepia" or dark brown appearance according to the light. On the flanks the colour is "smoke-grey" with a slight suggestion of "lavender-grey." The bellies, breasts, and underparts generally are whiter in the four females than in the old male, where they have a decided yellowish tinge.

The general colour of the three darker examples—all males of the "Old English" type, *Mus rattus rattus*—is dark "slate-grey" shading into "olive-grey" on the belly. There is a suspicion of brown but the black and grey hair so predominates that the brown is almost entirely

* See Table of Measurements at end of Paper.

** The colour terms used are taken from Ridgway's "Nomenclature of Colors," 1886.

lost. Mr. Millais describes the vibrissæ as black, but in these examples there are brown and grey hairs as well as black; his statement that the ears are naked is not correct; they are covered with short hair on both the outer and inner surface.

Even if we accept the statements of Grose and Chanter as evidence that *Mus rattus* was the Rat of Lundy prior to the date at which *Mus norvegicus* arrived, we have no proof that it is, as Chanter calls it, indigenous. Apart from the supposition, believed by most authorities to be proved, that the Black Rat has only inhabited England for a few centuries, I must call attention again to the fact that Black as well as Brown Rats constantly reach our ports. We have in these eight examples two distinct types and no intermediate forms, suggesting a question, which, however, we cannot answer without reference to a large range of specimens; do the two types on Lundy breed true? If this, in the future can be shown to be the case, we shall conclude that one or other or both types have quite recently been introduced, for in most cases where the sub-species live together we find these intermediate forms. A number of examples caught in the Liverpool Docks, which I examined, showed the brown upper parts of *Mus rattus alexandrinus* and the smoky under surfaces of *Mus rattus rattus*. The large range in the British Museum includes all manner of variations, and from the same locality are white and dusky bellied Alexandrine Rats. As a rule the older Rats show a yellow tinge on the belly, but though many young examples are very white underneath, some are distinctly dusky.

The statement, then, made by Mr. J. Ll. W. Page (6) that Lundy "is the last refuge of the Old English black rat, which still inhabits Rat Island; indeed till about forty

years ago it was the only species on the island," is misleading. The Black Rat occurs and has occurred regularly or occasionally in many localities in our islands ; it is not a native, and therefore reached Lundy on shipboard, where it has been noticed from time to time for at least 150 years, but the island can hardly be called its last refuge.

The melanic variety of *Mus norvegicus*, formerly known as *Mus hibernicus*, Thompson, was recorded by H. J. Charbonnier (7) ; he received an example in November, 1891, which had been shot on Lundy.

Rabbits are mentioned in the earliest accounts of Lundy ; in an Inquisition made in 1274, the annual take of Rabbits is estimated at 2,000, 5/6 per 100 skins being the value, as the flesh was not sold, and in an even earlier 13th century deed they are also mentioned as a valuable asset. Later we find frequent references to " Connies in great store," " conies very plentiful," and " stores of conies." They are still plentiful, especially on the west coast, where the thrift-grown turf is honeycombed with burrows. In the spring many of the burrows are the homes of puffins, but at the time of our visit the Rabbits were in sole possession.

Chanter's reference to Seals is as follows :—" Seals frequent the Island in considerable numbers. The only species identified is the common spotted seal, *Phoca vitulina.* No less than five have been killed at one time in the Seal Cavern, which is their principal place of resort ; but they have much diminished during the last few years, owing to their reckless persecution by the crews of pilot and tug-boats."

Page (6) refers to " *Phoca vitulina*, or the grey seal," and adds, " there is one fine old fellow who cannot be caught. He is called by the islanders ' Ponto,' and they say he is

'as large as a young horse.'" His use of the name "grey seal," coupled with the scientific name of the Common Seal, shows that, like Chanter, he was not aware that the larger species might occur, but unless "Ponto" was an island myth, like the Great Auk which was reported from Lundy, an animal "as large as a young horse," must have been *Halichœrus grypus* (Fabr.).

It is possible that the Common Seal occurs on the Lundy coast, but neither Chanter nor Page tell us by whom it has been identified. We saw three Seals, two basking on rocks immediately below the old fog-signal battery, and the third swimming near the same rocks, and on the same day others were observed at the north end of the island. One Seal remained on a stack until the rising tide washed over it, and we were able to examine it closely with our glasses; there was no doubt about the species, it was too large for a Common Seal, and had the flattened head and long snout of the Grey Seal, *Halichœrus grypus*. The second, though a smaller animal, was of the same species, and the third, which we only saw imperfectly, was, as it was with the others, probably a Grey. The Grey Seal is *the* Seal of the Scilly Islands; there is a colony on the Pembroke coast, and it is more of a cave and rock haunting species than the Common Seal. The caves of Lundy are suitable breeding haunts for this big Seal.

It is not easy to estimate the size of any animal, but we thought that the Seal on the stack would measure about 8 feet in length. When we first noticed it, it had evidently recently left the water, and looked very dark, but light markings appeared as its coat dried. It was mottled with blue-black and grey on the back, and was much lighter on the belly, which it exposed fully to view when it rolled over and dozed, lying on its back. Its

hind flippers were extended somewhat stiffly behind, their inner surfaces meeting, and when the animal was on its side they were raised considerably; its tail was held at right angles to the long axis of its body. The fore-flippers were frequently held or rubbed together, like a man rubbing his hands; occasionally one was waved in the air, or was used to scratch its face or other parts of its body. It was apparently annoyed by the splashing of the incoming tide, raising its head frequently and looking round, and shifting its position to a slightly dryer part of the rock, but it did not leave until the water was swirling round it. Then it bumped heavily along the rock until the fore part of its body overhung the water, when it slipped rather than dived into the sea, almost without a splash.

Mr. T. V. Wollaston, who twice visited the island to study the Coleoptera and published accounts of his visits in the "Zoologist" for 1845 and 1847, was struck with the similarity of its insect fauna to that of Wales; he found many beetles which at that time were only known to occur in Wales and had not been found in Devon (8). The later observations of Mr. F. Smith and the recent work of Messrs. Norman H. Joy and J. R. le B. Tomlin (9) have added much to our knowledge of the Coleoptera; many of the species unknown in Devonshire in the forties have since been discovered there, and Mr. Wollaston's theory is no longer accepted. The coleopterous fauna is, however, remarkable; Dr. Wallace (10), after mentioning the two beetles then known to be peculiar to the island—*Ceuthorhynchus contractus* var. *pallipes*, Crotch, and *Psylliodes luridipennis*, Kuts., remarks about them:—"Still more curious is the occurrence of two distinct forms (a species and a well-marked variety) on the small granitic Lundy Island in the Bristol Channel. This

island is about three miles long and twelve from the coast of Devonshire, consisting mainly of granite with a little of the Devonian formation, and the presence here of peculiar insects can only be due to isolation with special conditions, and immunity from enemies or competing forms."

It would be unwise, considering how slight is our knowledge of the mammalian fauna, to make definite statements, but if it can be proved that the Shrew of Lundy is the Lesser and not the Common Shrew and that there are no Voles, we shall, coupling this fact with its peculiar Coleoptera, conclude that the island has been long isolated. We naturally think of the conditions in Ireland and the Isle of Man, where the Lesser Shrew but not the Common Shrew is found and where Voles are absent, though on both these islands the sub-specific form of the Stoat occurs and on Lundy no Mustelid is known. The comparison of the sea-depth round these islands does not throw much light on the subject. The depth between Lundy and the English and Welsh coasts exceeds 20 fathoms, but between the Isle of Man and England it is less than 20 fathoms, whilst between Man and Ireland there is an ocean valley of over 50 fathoms. We cannot, however, estimate the ancient depth of the Irish sea east of Man, for it is silted, owing no doubt to the action of the meeting tides which are held responsible for the coast erosion in Morecambe Bay. Further researches in this and other branches of natural history would probably amply repay the worker, and add greatly to our knowledge of this isolated and interesting island.

MEASUREMENTS OF LUNDY BLACK RATS, *Mus rattus*, in mm.

No.	Sub-species.	Sex.	Head and Body.	Tail.	Hind foot.	Ear.
C. & O.						
2	*Mus rattus alexandrinus*	♀	198	227	36	24
3	„ „ „	♀	170	198	36	22
8	„ „ „	♀	177	196	38	22
9	„ „ „	♀	167	201	36	22
10	„ „ „	♂	199	234	37	25
5	„ „ *rattus*.....	♂	188	208	38	24
6	„ „ „	♂	209	233	40	25·5
7	„ „ „	♂	195	201	37	24·5
	Average	♂s	197·75	219	38	24·75
	„	♀s	175·5	205·5	36·5	22·5

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VII. The Atomic Weight of Chlorine.

By E. C. EDGAR, D.Sc.

(Received and read January 14th, 1908.)

The method used to redetermine this constant was to burn pure dry chlorine, at the tip of a quartz jet, in an atmosphere of pure dry hydrogen in a quartz "combustion vessel"; the hydrogen chloride formed was condensed in a limb of it by liquid air.

The weights of the gases burnt were found by subtracting from the total amounts used the weights of unburnt hydrogen and chlorine.

Eight experiments were made. In six the hydrogen chloride was distilled from the "combustion vessel" into a steel bomb and weighed as a liquid. In three of these the weights of the liquid were less than the weights of hydrogen and chlorine burnt; in the other three the bomb leaked.

In the last two the hydrogen chloride was distilled into an apparatus containing water and weighed as aqueous hydrochloric acid.

As the mean of eight experiments, the atomic weight of chlorine calculated from the ratio

$$\frac{\text{weight of chlorine burnt}}{\text{weight of hydrogen burnt}}$$

is 35·194; from the ratio

$$\frac{\text{weight of hydrogen chloride caught} - \text{weight of hydrogen burnt}}{\text{weight of hydrogen burnt}}$$

it is 35·193 (atomic weight of hydrogen = 1).

February 6th, 1908.

If the atomic weight of oxygen is taken as 16, that of chlorine becomes 35·462 and 35·461 respectively.

These numbers differ appreciably from that accepted by the International Committee on Atomic Weights, 35·45, but agree very well with 35·463 found by Dixon and Edgar in 1905, and with 35·461 lately proposed by Guye.

Manchester Memoirs, Vol. lii. (1908), *No.* 8.

VIII. On a New Type of Dynamical Stability.

By ANDREW STEPHENSON.

(Received and read January 28th, 1908.)

1. A system in a position of equilibrium, and capable of oscillation about that position, may be acted on by periodic force in such a way that no oscillation is generated. If, for example, one end of a vertical stretched string is moved to and fro in the direction of the length, this imposed motion has no tendency to produce lateral vibration. In certain cases, however, it has considerable effect in intensifying an already existing oscillation; in particular, if the imposed frequency is double that of the lateral motion, a very marked swing is magnified from the slightest initial disturbance.

Another example of a similar kind is afforded by a pendulum, the point of suspension of which is subject to a vertical periodic motion: if the frequency is double that of the pendulum, any small swing is gradually magnified by cumulative action.

Various instances of the double frequency effect forced themselves upon the attention of observers, and it appears to have been assumed—possibly from the simplicity of the phenomenon—that it is only in the case of double frequency that this type of disturbance has appreciable influence. Mathematical investigation has shewn, however, that the effect is cumulative in the whole series of cases when the disturbance frequency is approximately $2/r$ of the natural frequency of the system, r being any

March 5th, 1908.

integer.* The intensity of the magnifying effect falls off rapidly when r is taken larger, and as our terrestrial systems are always subject to friction, it is difficult to exhibit many cases experimentally. With the pendulum the intensifying influence may be observed for several values of r without special nicety of adjustment.

Our present object is to establish another very remarkable property of the non-generating type of periodic disturbance.

2. In the preceding pendulum experiments the resulting motion is due to the combined action of the imposed force and gravity. Let us enquire as to the effect of the imposed force acting alone.

To examine this question experimentally, a rod is pivoted vertically so that it is free to rotate in a horizontal plane: when the pivot is driven horizontally in a simple vibration along the length of the rod the relative equilibrium is not disturbed. If, now, the rod is displaced through a small angle, it is observed to swing about the line of the imposed motion in a period large compared with that of the pivot.

All the properties of the motion may be deduced from the differential equation determining it, but here we seek an approximate treatment of the problem, based on general mechanical considerations, in order to obtain a notion of what happens more vividly than is possible from the exact solution. For this purpose we assume that a, the amplitude of the pivot motion, is small, and also that the speed of the pivot, P, is constant, and equal to V, say, throughout the path; *i.e.*, we assume that the body is acted on by suitable impulses applied at P at the ends of its path, being free from action in all intermediate

* "On a class of forced oscillations," *Quart. Journ. Math.*, No. 148, 1906.

positions. We shall first obtain the magnitudes and directions of the impulses necessary to impose the motion.

Let P be the instantaneous centre of rotation of a body of mass M, and with mass centre C. If an impulse, I, acts at P in a direction making a small angle, ϕ, with PC, it gives the mass centre a velocity I/M, and at the same time produces an angular velocity $Ih\phi/Mk^2$, where $h=PC$ and k is the radius of gyration about C.

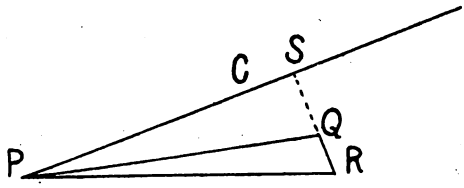

Fig. 1.

The velocity of P after the impulse is thus made up of the components $\overrightarrow{PQ}=I/M$ along the line of the impulse, and $\overrightarrow{QR}=(Ih^2/Mk^2)\phi$ perpendicular to PC: the resultant is therefore $\overrightarrow{PR}$. In our problem this velocity is constant (numerically), $=V$. PQR being approximately a right angle, PQ differs from PR by a small quantity of the second order: hence the impulse $I=M.PQ=MV$ so far as quantities of the first order are concerned. Also if RPC is denoted by θ

$$\frac{\theta}{\phi}=\frac{SR}{SQ}=\frac{h^2+k^2}{k^2}.$$

Thus to impress the required velocity V on P in a direction at an angle θ to the rod an impulse MV must act at an angle $\frac{k^2}{h^2+k^2}\theta$ to the rod. An impulse of double magnitude will then reverse the motion of P from V to $-V$: and the imposed vibratory motion of P with constant speed V in the path AB is produced by impulses

of magnitude $2MV$ applied in the direction dividing the angle between the rod and AB in the constant ratio k^2/h^2. It will be convenient to regard the action at either end of the path as made up of two consecutive impulses, MV, one bringing P to rest, and the second giving it the velocity towards the other end. The angular velocity at the instant between the two impulses is then equal to the mean angular velocity during the cycle of which the instant is the mid point.

Consider the motion from the instant when P has been brought to rest at A by the action of the first half-impulse; let the inclination of PC to AB be θ and the angular velocity ω. The moment of the second half-impulse about C is

$$MV\frac{k^2h}{h^2+k^2}\theta$$

and the resultant change in the angular velocity

$$Vh\theta/(h^2+k^2).$$

If τ is the time of motion to B the inclination of the rod at B is therefore

$$\theta+\{\omega+Vh\theta/(h^2+k^2)\}\tau,$$

and the moment of the impulse at B

$$-2MV\frac{k^2h}{h^2+k^2}[\theta+\{\omega+Vh\theta/(h^2+k^2)\}\tau].$$

The angular velocity after this impulse is

$$\omega-Vh\theta/(h^2+k^2)$$

correct to small quantities of the first order, and the inclination on reaching A again is $\theta+2\omega\tau$. Hence the moment of the first half-impulse bringing P to rest at A is

$$MV\frac{k^2h}{h^2+k}(\theta+2\omega\tau).$$

By summation the total moment applied during the motion of P from rest to rest at A is

$$-2MV\frac{k^2h^2}{(h^2+k^2)^2}\theta\tau.$$

Hence if ω' is the angular velocity at the end of this cycle,

$$\frac{\omega' - \omega}{2\tau} = -\left(\frac{Vh}{h^2 + k^2}\right)^2 \theta ;$$

i.e., the mean angular acceleration is directed towards the position of relative equilibrium, and is proportional to the angular displacement. The mean motion is therefore a simple oscillation

$$\theta = a \sin\left(\frac{Vh}{h^2 + k^2} t + \epsilon\right) \quad . \quad . \quad . \quad (1)$$

The actual motion is evidently of the nature shewn in the diagram (*Fig.* 2), in which the time is plotted hori-

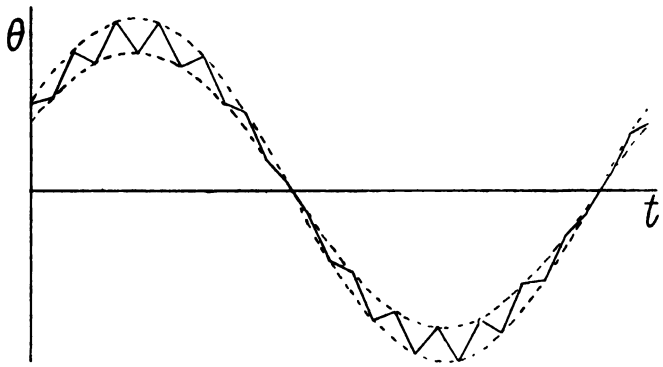

Fig. 2.

zontally and the angular displacement vertically: the two boundaries are sine curves and the successive vertices are equidistant in time.

The preceding synthetic investigation brings out the essential characteristics of the motion. The impulses are constant in magnitude, and the effect of any impulse in changing the angular velocity is proportional to the angular displacement; secondly, the angular displacement at B algebraically exceeds half the sum of the two at A on either side, by an amount proportional to the displace-

ment; whence it follows that the impulsive moment in any cycle from rest to rest at A is always directed towards the central line of motion AB, and varies directly as the displacement—the conditions determining a mean motion of simple oscillation.

Such considerations tend to familiarise the motion, but the quantitative results may be obtained by a more analytical method, which will now be given for the sake of comparison.

Let θ be the inclination of PC to AB on starting from A after the impulse, and ω the angular velocity at the same time*; also let θ_1, ω_1 be the values of these quantities after the impulse at B, and θ_2, ω the values after the succeeding impulse at A.

On starting from A the velocity of the mass centre has components

$$(V - h\omega\sin\theta,\ h\omega\cos\theta)$$

along and perpendicular to AB. After the impulse at B the velocity is

$$(-V - h\omega_1\sin\theta_1,\ h\omega_1\cos\theta_1).$$

Hence if $(X,\ Y)$ the impulse at B

$$X/M = -2V - h(\omega_1\sin\theta_1 - \omega\sin\theta)$$
$$Y/M = h(\omega_1\cos\theta_1 - \omega\cos\theta).$$

Again, considering the motion about the mass centre we have

$$Mk^2(\omega_1 - \omega) = (X\sin\theta_1 - Y\cos\theta_1)h$$
$$\therefore\quad k^2(\omega_1 - \omega) = -2Vh\sin\theta_1 - h^2(\omega_1 - \omega)\cos(\theta_1 - \theta),$$

and to the required degree of approximation

$$(h^2 + k^2)(\omega_1 - \omega) = -2Vh\theta_1 \quad . \quad . \quad . \quad \text{(i.)}$$

Similarly, considering the succeeding impact at A we have

$$(h^2 + k^2)(\omega_2 - \omega_1) = 2Vh\theta_2 \quad . \quad . \quad . \quad \text{(ii.)}$$
$$\therefore (h^2 + k^2)(\omega_2 - \omega) = 2Vh(\theta_2 - \theta_1) \quad . \quad . \quad \text{(iii.)}$$

* It must be remembered that ω here has not the same meaning as in the preceding.

Now, during the cycle, the mean angular velocity

$$= \frac{1}{2}(\omega + \omega_1)$$

$$= \omega - \frac{Vh}{h^2+k^2}\theta_1$$

$$= \omega\left(1 - \frac{Vh}{h^2+k^2}\tau\right) - \frac{Vh}{h^2+k^2}\theta \quad . \quad . \quad . \quad \text{(iv.)}$$

Similarly, during the succeeding cycle, the mean angular velocity

$$= \omega_2\left(1 - \frac{Vh}{h^2+k^2}\tau\right) - \frac{Vh}{h^2+k^2}\theta_2 \quad . \quad . \quad . \quad \text{(v.)}$$

Therefore, from (iv.) and (v.), the change in mean angular velocity

$$= (\omega_2 - \omega)\left(1 - \frac{Vh}{h^2+k^2}\tau\right) - \frac{Vh}{h^2+k^2}(\theta_2 - \theta)$$

$\therefore$ from (iii.)

$$= \frac{Vh}{h^2+k^2}(\theta_2 - 2\theta_1 + \theta) - 2\left(\frac{Vh}{h^2+k^2}\right)^2(\theta_2 - \theta_1)\tau$$

$$= \frac{Vh}{h^2+k^2}(\omega_1 - \omega)\tau - 2\left(\frac{Vh}{h^2+k^2}\right)^2(\theta_2 - \theta_1)\tau$$

$\therefore$ from (i.)

$$= -2\left(\frac{Vh}{h^2+k^2}\right)^2(\theta_1 - \theta_2 - \theta_1)\tau$$

i.e. $$\frac{\text{change in mean angular velocity in cycle } 2\tau}{2\tau} = -\left(\frac{Vh}{h^2+k^2}\right)^2\theta$$

when small quantities of the second order are neglected.

Thus the mean angular acceleration is directed towards the position of equilibrium, and is equal to $\left(\frac{Vh}{h^2+k^2}\right)^2 \times$ the angular displacement—the result obtained previously.

It is of interest to compare this case of constant pivot speed with the motion when the pivot has a simple vibra-

tion of small amplitude a, and frequency n per 2π units of time. The equation of motion is then

$$\ddot{\theta} + \frac{h}{h^2+k^2} an^2 \cos nt\, \theta = 0$$

and the solution* when $ah/(h^2+k^2)$ is small, is approximately

$$\theta = a\sin\left(\frac{1}{\sqrt{2}} \frac{anh}{h^2+k^2} t + \epsilon\right) \quad . \quad . \quad . \quad (2)$$

It is evident that (1) and (2) are of the same type, an in (2) being the maximum velocity of P in its path.

We have thus proved that when the amplitude of the pivot motion is small, the body swings in a simple vibration of frequency proportional to the frequency and to the amplitude of the applied motion.

3. Now consider a body free to rotate about a horizontal pivot, and set in the position of unstable equilibrium: what will be the effect of a vertical oscillation of the pivot on the stability of the equilibrium?

In the position inclined to the vertical at an angle θ, the mean angular acceleration due to the imposed motion is $\frac{1}{2}\left(\frac{anh}{h^2+k^2}\right)^2\theta$ inwards, from (2); while the outward acceleration due to gravity is $\frac{gh}{h^2+k^2}\theta$. The resultant is therefore

$$-\left\{\frac{a^2n^2h}{2(h^2+k^2)} - g\right\}\frac{h}{h^2+k^2}\theta$$

and the acceleration is always towards the vertical if

$$(an)^2 > 2g(h^2+k^2)/h.$$

Thus the inverted pendulum is rendered stable by a small simple vertical oscillation of the pivot of maximum velocity greater than

$$\sqrt{2g(h^2+k^2)/h}.$$

* The investigation is given in a paper "On Induced Stability," *Phil. Mag.*, February, 1908.

When this condition is satisfied, the motion about the vertical is simple vibratory of frequency

$$\mu = \sqrt{\left\{\frac{a^2n^2h}{2(h^2+k^2)} - g\right\}\frac{h}{h^2+k^2}} \quad . \quad . \quad (3)$$

per 2π units of time.

To illustrate these results experimentally, a uniform rod of length 39·6 cm. was pivoted at one end, and the pivot was moved in an approximately simple vibration of amplitude 3·85 cm. With an applied motion of frequency 11·2 per sec., the period of the small oscillations about the vertical was found to be 1·64 sec. The above formula, (3), gives 1·58 sec. The 4% difference may be attributed partly to the effect of friction in lengthening the period, and partly to error in the determination of the pivot frequency.

If the imposed motion is slightly inclined to the vertical, it is observed that the pendulum makes small oscillations about a position much more markedly oblique in the same direction. This effect can be explained very simply.

Let the inclination of the applied motion be β, and that of the mean position of the pendulum γ. The accelerations due to the applied motion and gravity in this position must be equal and opposite; *i.e.*,

$$\tfrac{1}{2}\left(\frac{anh}{h^2+k^2}\right)^2(\gamma-\beta) = \frac{gh}{h^2+k^2}\gamma,$$

and therefore

$$\gamma = \beta \Big/ \left\{1 - \frac{2g(h^2+k^2)}{a^2n^2h}\right\}$$

Thus γ is large compared with β when n is near the limit necessary for stability.*

Finally, it may be noted that the stability effect still

* For the determination of the amplitude of the forced oscillation about the mean position, reference may be made to § 1 of the paper on "Induced Stability" already quoted.

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Thus γ is large compared with β when n is near the limit necessary for stability.*

Finally, it may be noted that the stability effect still

* For the determination of the amplitude of the forced oscillation about the mean position, reference may be made to § 1 of the paper on "Induced Stability" already quoted.

holds when the rod is supported by a double joint so that it has complete freedom of motion about the vertical.

4. The particular case of dynamical stability investigated above is an example of a general type. If any system fixed by one coordinate is statically unstable in a position of equilibrium, that position is rendered stable by the action of a periodic disturbance applied in such a way as to generate no motion about equilibrium. The maintenance of the stability here does not necessarily demand an impressed motion: in the case of the inverted pendulum, for example, a periodic variation in gravity would have the same effect as the vertical oscillation of the pivot.

Some types of steady motion are also rendered stable by a non-generating periodic disturbance*; thus a top rotating at a speed too low for stability is maintained about the steady state by a vertical oscillation of the point of support.

It is possible that this method of ensuring the stability of a steady motion may be of service in special cases where the more usual devices are not applicable. In the problem of mechanical flight the great difficulty lies in obtaining longitudinal stability at slow speeds; if an aeroplane system is started in steady motion, a small disturbance results in a pitching oscillation, which continues with increasing violence until finally the glider is overturned. The mathematical investigation of the effect of the non-generating periodic disturbance which is illustrated in this paper, was undertaken with the view of its possible application to a mechanism whereby stability in gliding would be automatically ensured. In such a case the motion is of a more complex character, involving the interaction of several co-ordinates: it is hoped to give a general examination of the problem later.

* *loc. cit.*, § 2 and 3.

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* *loc. cit.*, § 2 and 3.

Manchester Memoirs, Vol. lii. (1908), *No.* 9.

IX. "A Method of Counting the Number of α Particles from Radio-active Matter."

By Professor E. RUTHERFORD, F.R.S.,

AND

H. GEIGER, Ph.D.

(Received and read February 11th, 1908.)

The total number of α particles expelled per second from one gram. of radium has been estimated (Rutherford *Phil. Mag.*, Aug. 1905) by measuring experimentally the total positive charge carried by the α rays from a thin film of radium, on the assumption that each α particle has the same charge as an ion produced in gases. If the α particle is an atom of helium, it is necessary to assume that each α particle carries twice the ordinary ionic charge. The need of a method of directly counting the number of α particles shot out from radio-active matter has long been felt in order to determine with the minimum of assumption the charge carried by the α particle and also the magnitude of other radio-active quantities.

It can be calculated that an α particle expelled from radium produces about 80,000 ions in a gas before its ionizing power is lost. With very sensitive apparatus, it should be just possible to detect the ionization produced by a single α particle by electrical methods. The effect, however, would be small and difficult to measure with accuracy. In order to overcome this difficulty, we have employed a method which automatically increases the ionization produced by an α particle several thousand times and so makes the electrical effect easily observable

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with an ordinary electrometer. This is done by making use of the property discovered by Townsend, that an ion moving in a strong electric field in a gas at low pressure, produces a number of fresh ions by collision with the gas molecules. If the electric field is adjusted nearly to the value required for the passage of the spark, a single ion generated in the gas by external agencies, produces in this way several thousand fresh ions by collision. In the experimental arrangement, the testing vessel consists of a brass tube 60 cms. in length, along the axis of which passes a thin insulated wire attached to the electrometer. With a gas pressure of about 2 cms. a potential difference of about 1,000 volts between the brass tube and the wire is required. The α particles are fired down the tube through a small hole at the end of the tube about 2 mms. in diameter covered with a thin plate of mica. In order to use a narrow pencil of α rays, the active matter in the form of a thin film on a surface about one square cm. in area is placed in an exhausted tube which is a prolongation of the testing vessel. The distance of the active matter from the hole is usually between 50 and 75 cms. and the amount of active matter adjusted so that from six to ten α particles are fired through the hole per minute. The effect of the α particle entering the testing vessel is shown by a sudden throw of the electrometer needle. Under good conditions, this throw is about 50 divisions using an electrometer which has a sensibility of 300 divisions per volt. By observing the number of throws of the electrometer needle, we can count the average number of α particles shot through the opening per minute. The total number fired out by the active matter can be calculated from the known area of the opening and the distance of the latter from the active matter. Preliminary observations show that the number

of α particles counted by this method is of the same order as the calculated number, but special experiments are in progress to determine with accuracy the value of this important constant. By counting at intervals the number of α particles expelled per minute, we have been able to obtain the curves of decay of activity of a plate coated with radium *C* or actinium *B*.

The α particles from a constant source are shot out at irregular intervals. The time interval between the entrance of successive α particles has been observed over a long interval, and the results show that the distribution curve with time is similar in general shape to the probability curve of distribution of the velocity of molecules in a gas. Further observations, however, are in progress to determine the distribution curve with the accuracy required for comparison with the mathematical theory.

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THE WILDE LECTURE.

X. On the Physical Aspect of the Atomic Theory.

By Professor J. LARMOR, Sec.R.S.

Delivered March 3rd, 1908.

When Descartes proceeded methodically to shake himself free from the trammels of the scholastic philosophy, and to reconstruct the content of his knowledge on what is essentially the modern basis, he found among other things—some of them now fantastic—that it was unintelligible to suppose that matter could act where it was not, *i.e.*, could produce an influence in regions with which it was not in continuous structural connexion. So powerfully did this feeling dominate his thought, that he appears to have been unable to form any conception of mere empty space as distinct from some mode of occupation of it: to him, space was a *plenum*, the seat of the processes of communication between the sensible objects which it contains. Direct interaction of these objects across distances, with mere nothingness between, was not a satisfying account of the relations between the apparently discrete masses which constitute our sensible universe.

The modern idea of an aethereal medium, as a means of transmission of physical influences from mass to mass, receives its first systematic exposition in his physical writings. The Sun is for him the centre of a great aethereal vortex, by which the planets are swept round in their orbits; light consists of impulsion or pressure propagated through the fluid *plenum*. An attempt is

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carried through to reconstruct the phenomena of physics and physiology on a basis of mutual connexion—thereby starting afresh the aspiration which is fundamental to all scientific instinct, the effort to push to the utmost the unravelling of rational foundations of the scheme of things in which we subsist. And, whereas in Descartes' time there was little to go upon, except the imagination applied to the common facts of experience, now there are the vast and growing accumulations of ascertained experimental knowledge in the various Sciences, affording a most urgent stimulus towards the continued cultivation and improvement of general syntheses; these in turn react as the ever present incitement to the further pursuit of scientific experiment into regions economically devoid of profit.

The Cartesian system of celestial vortexes had been absorbed into common modes of thought, as a natural and intelligible feature in the cosmogony, when the precise observations of Kepler and the deductions of Newton came to replace this obvious mode of representation by new but exact principles whose foundations were entirely concealed from view. It is not easy for us now to imagine how strange must have been the idea that the planets were drawn inward to the Sun by a direct pull across space, depending on nothing but their distance apart,—by a force which was postulated to act quite irrespective of whatever obstacles might intervene between them. To prepare for the unimpeded operation of direct forces across space such as Newtonian gravitation, the aether of Descartes—resisting medium so-called in this connexion—which had to carry round the planets in its vortex, must be rigorously abolished: and space appears again as empty. Even the intellect of Huygens—whose vast range of achievements has been largely

masked by the contemporary presence of Newton—seemed unable to accept the new doctrine in its full scope. To a mind like his there could remain no question about the chief Newtonian deductions: the evidence was conclusive that the interactions through the aether of bodies far apart in comparison with their magnitudes, and with nothing between, must somehow adjust themselves into the gravitational law of attraction. But he was unable to understand how the complex mutual influence of masses near together could possibly, in all cases without exception, resolve itself into a result so uniform and so simple. And to this day we remain largely in the position of Huygens with regard to this subject. The evidence, which in its beginnings enabled the genius of Newton to detect and develope his cosmical system, has of course long ago become overwhelming. Yet why is the gravitational attraction of a particle of matter sunk at the centre of the Earth entirely unaffected by all the intervening mass? We do not know, any more than Newton did, how this action is transmitted. We may take refuge in the idea that the nuclei in the aethereal medium, which constitute the cores of the fields of activity known to us as material atoms, must even in the densest matter occupy a space absolutely infinitesimal compared with the whole region of aether, and so not obstruct or modify the transmission of the gravitative influence. To get play for rational conceptions, we are thus thrown back on the atomic theory of matter, and that in its more modern physical aspect, to which it is now time to pass on.

It does not appear that Descartes was able to penetrate to any idea of the relation of the cosmical vortex to the atoms of the material bodies which it carried round in its grasp; they were merely like ex-

traneous dust or mist whirled in the wind. For an adequately exact type of unifying conception of the relation between matter and aether, of their structural connexion, science had to wait until the middle of the nineteenth century. The profound analysis of Helmholtz had revealed the unexpectedly simple scope of the principles determining the interactions of vortexes in fluid—one of the most brilliant of the achievements of mathematical reasoning, whose highest function must always be to condense the unmanageable mass of relevant particulars into the practicable limits of general principles. His main result was that, in the entire absence of friction in the fluid, each vortex ring would be a permanent state of motion, capable of temporary modification (distortion, vibration, etc.), through interaction across the fluid with other vortexes which come within its range, but always in the end recovering its original condition, and thus retaining its individuality through unlimited time. We can well imagine the keen interest excited by Lord Kelvin's rapid *aperçu* that such vortexes may represent in essential features the atoms of matter. For here we have a type of atom that is not something foreign to the aether, merely immersed in it and pushed about by it, but a permanent located whirl or stable state of motion which subsists in the aether itself, and is of its very essence.

Here was suggested a mode of unification of a duality previously unresolvable ; and we can appreciate the zeal with which the problem of the investigation of this vivid image of one of the fundamental modes of interconnexion in the scheme of nature was attacked by mathematical physicists. Its development, in which alongside the name of Helmholtz that of Lord Kelvin will ever stand, has constituted, as we all know in this

place, a new science, that of abstract hydrodynamics, which analyses the interaction of uniform inertia and simple fluid pressure, and is in itself one of the most elegant and perfect constructions of modern mathematics. As a result of these thorough investigations, we are now more familiar with the limitations of the so-called vortex theory of atomic constitution, than with the initial successes of this mode of representation of physical reality. But limitation ought not to be taken to imply failure. Human reason is finite in its potentialities: it is not competent to frame a picture of the activities of the *cosmos*, of which it is itself a part, such as can bear comparison with actuality throughout the whole range of phenomena. It is of course absurd, as we are often reminded with much insistence, especially in recent years, to imagine that a material atom is merely a vortex ring in fluid; but, on the other hand, we can never know any object, even an atom, intrinsically, but only through its relations with other objects, and the picture of atoms as motional configurations subsisting in some way in the universal *plenum*, and not merely objects foreign to it, is possibly the greatest expansion which our modes of thought on these matters have received since Descartes; and it has come to us, or has at any rate been made definite, through the vortex illustration.

This procedure, the study of the relations of the universe by the construction of working models, easily apprehended as a whole, is not restricted to external nature. A considerable part of present activity in abstract mathematics seems to consist in the construction of schemes of representation, such as will elucidate the inner scope and connexion of the processes of mathematical thought, involving analysis of the ideas of number and magnitude, limit, infinity, etc. It is signifi-

cant that in both cases some kind of atomism has been a mental necessity, as it was in the earliest Greek inquiries.

We have recalled that, in order to make way for the principle of gravitation, Newtonians were compelled to clear the celestial spaces of the "resisting medium" which constituted the aether of Descartes. But that by no means implied any belief that gravitation did not require a medium for its transmission. Towards the end of his life, Newton allowed himself to set down formally in the famous series of "Questions" appended to the second edition (1717) of his "Opticks,"* his speculations on this and related subjects concerning the constitution of matter, pervaded as they were by constant suggestion of the vibratory motions which constitute heat, the radiation which these motions excite, and their close relation to chemical change. His ideas (Query 17 *seq.*), of essentially modern type, involved a medium infinitely more rare than air and of infinitely stronger elasticity—*aether* is his own name for it—amidst the waves of which the atoms of matter and the corpuscles which he took to constitute light were agitated like logs in a sea; of such a medium the elastic pressure, weaker on the adjacent sides of bodies, might, as he thought, in some way represent gravitational attraction, while its dead resistance to planets moving through it would be, owing to its small mass, quite negligible.

At the end of a prolonged physico-chemical discussion he sums up his atomic view of the constitution of matter (*loc. cit.*, p. 375) in archaic terms with deep modern significance, that have often been quoted:

"All these things being consider'd, it seems probable to me that God in the Beginning form'd Matter in solid, massy, hard,

* "Opticks," ed. (3), pp. 313—382.

impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as more conduced to the End for which he form'd them; and that these primitive Particles being Solids, are incomparably harder than any porous Bodies compounded of them; even so very hard as never to wear or break in pieces: No ordinary power being able to divide what God himself made one in the first Creation. While the Particles continue entire, they may compose Bodies of one and the same Nature and Texture in all Ages: But should they wear away, or break in pieces, the Nature of Things depending on them would be changed. Water and Earth composed of old worn Particles and Fragments of Particles, would not be of the same Nature and Texture now, with Water and Earth composed of entire Particles in the Beginning. And therefore that Nature may be lasting, the Changes of corporeal Things are to be placed only in the various Separations and new Associations and Motions of these permanent Particles: compound Bodies being apt to break, not in the midst of solid Particles, but where those particles are laid together and only touch in a few Points.

"It seems to me farther, that these Particles have not only a *Vis inertiae*, accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles [Energies], such as that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies. These Principles I consider not as occult Qualities, supposed to result from the specifick Forms of Things, but as general Laws of Nature, by which the Things themselves are form'd; their Truth appearing to us by Phænomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult....."

This survey carries us about as near as purely physical speculation, based on the broad simple principles of universal dynamics that Newton was the first definitely to codify, could approach towards an atomic theory. And it is a considerable advance. The uninstructed tendency, judging from one's own early recollection, is to assume

that the apparently continuous substances around us are divisible without limit, and afterwards to wonder what sort of evidence it is that has suggested the contrary conclusion. Such evidence must have an essentially chemical flavour: its gist has been strikingly expressed by Newton in the argument of which the conclusion has been quoted. It is true that the mathematical physicists of the beginning of the nineteenth century were accustomed to conduct their investigations in terms of atoms or particles of bodies: but for their purposes, these terms were hardly much more than the embodiment of the fact that exact reasoning requires numerical expression, and therefore, the mathematical resolution of the media with which it is concerned into infinitesimal geometrical parts.* On a rather higher plane must however be placed the (subsequent) electric atoms of W. Weber, which may be held to have been somewhat unduly discredited by the destructive criticism of Helmholtz.

The Daltonian Atoms.

Early in the nineteenth century the time had come for the translation of these dim physical perceptions into secure experimental knowledge. Perhaps the new feature

*An exception must be made at any rate in the case of Young, as will appear in connexion with optical dispersion, and, as Lord Rayleigh has remarked, in connexion with capillarity. In a letter to Arago (Jan. 12, 1817), he reports:—"I have been reconsidering the theory of capillary attraction and have at last fully satisfied myself with respect to the fundamental demonstration of the general law of superficial contraction, which I have deduced in a manner at once simple and conclusive from the action of a cohesive force extending to a considerable number of particles within a given invisible distance. This solution has very unexpectedly led me to form an estimate, something more than merely conjectural, though not fully demonstrative, of the magnitude of the ultimate atoms of bodies; in water for instance, about" 10^{-9}cm. in diameter [modern estimates being nearer 10^{-8}cm.]. Young's "Works," ed. Peacock, vol. i., p. 382; also article "Cohesion," *loc. cit.* p. 462.

developed by Dalton is at bottom describable as the principle of the essential homogeneity of each pure substance, that it is composed of molecules of only one type, absolutely alike. Once it is postulated that only one kind of aggregation into molecules occurs, *e.g.*, that in water there is only one way in which the hydrogen attaches itself to the oxygen, the laws of definite and multiple proportions are self-evident. The only way to ascertain the truth of this hypothesis was to test the consequences experimentally. In the hands of Lavoisier it had become clear that in chemical transformation mass does not to a sensible extent ever disappear or re-appear, that chemical operations are not attended by dissipation or destruction of matter. In the hands of Dalton it became clear that each type of substance is characterized by its own specific type of aggregation of constituent atoms, by its own molecule.

At that time neither principle could have stood out in the full light in which we are accustomed to view it now. Physical ideas had retrograded since Newton's day. The heat which to his view seemed so obviously to be vibratory motion, due to the clash of atoms under their specific energies, had come to be regarded since Stahl, aided perhaps by a misreading of Black's doctrine of specific and latent heat, as a substance combined in various proportions with different bodies, on the idea that it is only something material that could be conserved.

And, moreover, as in all fundamental advances, the result attained was not so much the vindication of any inflexible experimental fact, as the introduction of an abstract guiding principle into the Science, fortified of course by experimental support. For it is still a legitimate aim of experiment to try whether any detectable change, either in mass or in gravity, is produced by that

re-aggregation of atoms to form new molecules, which constitutes chemical reaction. Some day the ascertained fact, that such influence of the close partial superposition of the fields of energy of the adjacent atoms in the molecule is at any rate almost infinitesimally small, may play a part in the elucidation of the mode of operation of gravitation. The essentially cognate fact, that no intervening obstacle can modify sensibly the gravitation of two masses, has already drawn us towards the position that the nuclei of the fields of stress, which constitute the physical aspect of atomic forces, are excessively small compared with the distances apart of these interacting nuclei in the fields of activity which are the atoms.

In the same way the Daltonian principle, of a definite molecule for each substance, now stands in intimate connexion with the Berthollet idea of statistical or mobile equilibrium, which requires that in the active interchange that is always going on among ultimate constituents of a substance, all the possible types of molecules which have any degree of stability must be present in some amount, though in most cases practically infinitesimal. It has also to take cognizance of fundamental considerations of a biological character, which will be referred to later.

While theory is aimless and impotent without experimental check, experiment is dead without some theory, passing beyond the limits of ascertained knowledge, to control it. Here as in all parts of natural knowledge, the immediate presumption is strongly in favour of the simplest hypothesis; the main support, the unfailing clue, of physical science is the principle that, nature being a rational *cosmos*, phenomena are related on the whole in the manner that reason would anticipate.

Radiation.

In sketching the progress of the purely physical

notion of atomic structure there is also another fundamental order of ideas, arising from the phenomena of radiation, which must be included—the conception of an atom or molecule as a vibrating system of some sort, complete in itself and reacting by resonance with such waves of radiation as have periodic times adjacent to the periods of its own free vibration.

In the famous memoir "On the Theory of Light and Colours" read by Thomas Young before the Royal Society on November 12th, 1801, which, in the form of a mass of brief and pregnant suggestions, lays the foundation of modern physical optics, the view of the refraction of light, as due to the reaction of natural free vibrations of the constituent parts of the refracting medium, had already been advanced. The passage perhaps demands quotation.* After giving a correct *aperçu* of the mechanism of total reflection, as involving and being supported by surface waves in the rarer medium, he proceeds as follows :—

> "*Proposition VII.* If equidistant undulations be supposed to pass through a medium, of which the parts are susceptible of permanent vibrations somewhat slower than the undulations, their velocity will be somewhat lessened by this vibratory tendency; and, in the same medium, the more, as the undulations are more frequent.
>
> "For as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer; and this retardation will be more frequent and more considerable as the difference between the periods of the undulation and of the natural vibration is greater."

It is hardly possible to extract definite meaning from this cryptic explanation: indeed, the dynamics of a com-

* "Lectures on Nat. Phil.," quarto edition, vol. ii., p. 623.

pound vibrating system is nowhere treated by Young; but, at any rate, he would have been quite prepared to predict the modern phenomena of anomalous optical dispersion as arising from the sympathetic vibrations of the molecules of the material medium. Later (1817), on again taking up the subject of Physical Optics, in his Article in the *Encyclopædia Britannica* on "Chromatics," he seems to have been dominated by the new puzzles connected with his principle of polarization due to transverse vibration, and this mode of explaining dispersion is dropped.

What are these parts of bodies—solid, liquid, or gaseous—which are thus taken to be susceptible of permanent vibrations of their own? At any rate, the hypothesis implies a thoroughly discrete structure of matter. And it is perhaps remarkable that Young was not tempted to ascribe the slowing of the period belonging to a given wave length to the mere loading of the aether by inert structureless particles of matter, in the manner of the explanation of dispersion which Cauchy and Poisson afterwards imposed on optics; he went to the root of the question, in the Newtonian manner (as he remarks in the appended Corollary and Scholium), by ascribing to these particles free periods of intrinsic vibration, and therefore definite and identical structures.

Nothing further is heard of this point of view until the origin of the lines of the spectrum, and the mechanism of the production of the dark Fraunhofer lines, were discussed between Stokes and W. Thomson in 1854.* In 1860, in introducing the practical discovery of spectrum analysis by Kirchhoff and Bunsen to the notice of the British public, Stokes based his dynamical explanation of selective absorption of radiation on the simple remark that, when the waves passing through the medium are

* G. G. Stokes, "Math. and Phys. Papers," vol. 4, 1904, appendix.

closely attuned to it, so as to induce strong sympathetic vibration in the parts of the medium which they traverse, the radiant energy emitted by these active vibrators must be supplied from that of the exciting train of waves.

But it appears that, in following out this chain of ideas on the nature of optical absorption, it had not occurred to either Stokes or Thomson to consider the reaction exerted by the vibrating absorber on the train of waves; and the elucidation of the dispersion of colours in light as due to the induced vibrations of the molecules, which had naturally presented itself to Young at the beginning of the century, remained to be enforced by Maxwell, Rayleigh,* Ketteler, and especially Sellmeier, having been as it seems definitely perceived and sought for experimentally, in the form of anomalous dispersion near a region of absorption, by the latter, as early as 1866. As the matter presented itself in luminous brevity to Lord Rayleigh, the medium is capable of free standing vibrations after the manner of a plucked string, executed in a periodic time which is determined for each type of vibration, *i.e.*, wave-length, by its elasticity and inertia alone, provided it contains no independent internal vibrators that could be excited cumulatively by this motion: but where it contains structures that can set themselves vibrating in sympathy, the circumstances are analogous to those of an oscillating string on which light free pendulums with periods of their own, near those under consideration, are hung: the regular swing of the system now takes place in a modified time, and the velocity of propagation, determined by the ratio of wave-

* See an early paper by Lord Rayleigh, "Scientific Papers," pp. 141—6 (1872)—prior to the publication of Sellmeier's demonstration, but, as he now thinks, possibly written with recollections of Maxwell's ideas—in which the practical inadequacy of mere differences of passive optical density is insisted on.

length to periodic time, must be altered, precisely in the manner that optical observation confirms.

The atomic constitution of matter is thus involved, in a highly refined manner, in the group of phenomena connected with the chromatic dispersion and absorption of radiation. But to gain conditions of ideal simplicity, we must attend to the case of gases where each molecule is isolated and free from encounter with others during the period required for thousands of its optical vibrations, where in fact it can be treated as vibrating free. The sympathetic vibration and resulting absorption along the spectrum, in liquid and solid bodies, are influenced in complex ways by the transient combinations of the molecules into groups: though the general relations elucidated by experiment between specific mean refraction and chemical structure suggest something often approximating to mere simple aggregation.

The Function of Conceptual Models.

In the case of every successful scientific theory, the time must come when its first easy triumphs become exhausted, and what prominently confront the investigator are its outstanding defects and difficulties. When this stage arrives, one way of saving appearances is to purify the theory by banishing all terms which have an illustrative or analogical connotation, expressing its verified relations alone by new words which represent simply general types of mathematical quantity—vectors, scalars, rotors, etc. When such a state of crystallisation has fully set in, further progress in general views is hardly to be hoped for—the sources of invention are dried up—though details in such a restricted abstract scheme will continue to be filled in, while new phenomena will probably suggest arbitrary unexplained additions to its content.

Even in chemical philosophy it has at times been a matter of concern that, for example, water is described as containing oxygen and hydrogen, whereas really it retains precisely none of the properties of either of these substances. Though it be admitted that it is constituted of molecules, yet the molecule of water is something different from its constituents; and it is held to be a crude or even unwarranted image that suggests that in it an oxygen atom and two hydrogen atoms lie alongside either at rest or in orbital motions. Criticism like this attaches to all inferences that cannot be tested by direct sensual perception. What we can know in any direct manner about chemical combination is expressed merely in the laws of definite and multiple proportions. Such a revision of the mode of expression of our knowledge as this criticism suggests may be useful occasionally as a stock-taking; but the misconceptions which it guards against are seldom real, and indeed it makes little, if any, permanent appeal on the physical side of the science. Here almost everything has been constructed on the basis of dynamical ideas,—those fundamental Newtonian ideas of force and inertia which constitute the simplest formal scheme that admits of permanence of free motions,—applied to conceptual models; such a theoretical representation is never perfect or complete, but it is vivid and illuminating, and historically it has been progressive; to give it up would be to replace a growing system by a collection of fragments of knowledge. The physicist in his own range is never likely to forget that any simple piece of matter is a vast interlacing, interdependent complex, which he can never hope completely to disentangle or resolve: he is certain that matter is of grained structure, but to him the grains are very far from being mutually isolated things,—each of them is actively influenced by all the others around it.

Yet he has no alternative but to hold that each ultimate grain is itself a self-existing cosmos, of complexity probably beyond any complete analysis on our part, which may indeed to appearance merge itself in combination with another atom or molecule, but is always recoverable unaltered,—that there is no degradation of matter. He holds probably that it is necessary to believe that in the same pure substance the molecules are all exactly alike, or, at any rate, that they are as nearly alike as individuals of a very sharply defined species in the organic world; though he knows no natural reason which would compel them to be so constituted, except in so far as they may represent the limited number of types of dynamical structures that can be built up from simpler identical primordial elements. It is vastly more suggestive to accept this wonderful inference, which constitutes the Daltonian theory, as our working hypothesis, than to try to refrain altogether from analogical reasoning about unseen molecules: moreover, this procedure is almost imposed *a priori* by the general principle already alluded to, that the simplest theory is probably the most fruitful representation of reality.

There is one branch of actual observational knowledge in which this identity of the molecules of a substance asserts itself with special strength: if the molecular theory had not been introduced on the evidence of the laws of definite and multiple proportions in chemical compounds, it must have demanded recognition as a result of a study of the crystalline structure of bodies. We call to mind that correspondences are now coming to light by which it is becoming possible to reason regarding the type of the molecule, and the geometrical grouping of its constituent atoms, from measurement of the crystalline aggregate: in such cases the single

molecule would itself be the ultimate formative crystalline element. Where an atom has a higher valency, it must, according to any formula of spacial chemical constitution, aggregate more atoms around it and in touch with it in the molecule: it must, on that account alone, itself occupy or exist in a larger central space. In this way greater atomic volume would be in general a result of greater valency, while the atomic volume will always be nearly the same in similar surroundings: the very striking recent investigations to ascertain how far the structure of the crystal is determined by the arrangement of the atoms in its molecule on the basis that equi-valent atoms require about the same atomic volume, are known to all of us here.*

The contrast has recently been sketched by Professor Voigt in eloquent terms between this domain of the properties of crystals, where all is definite, orderly arrangement, and that of liquids and gases where physical properties are merely average values which belong in the statistical sense to crowds of jostling molecules. But even here of course the regularity is limited; the molecules become confined more or less securely in definite positions by the mutual forces of cohesion, but not so firmly as to prevent them from taking part in the conduction of heat and other modes of equalisation or dissipation of energy; the very bonds of cohesion are themselves functions of the temperature.

The tendency of most physicists would still probably be to take comfort from a remark of Helmholtz, published in one of his letters, to the effect that organic chemistry progresses steadily and surely, but in a manner which, from the physical standpoint, appears not to be describable as quite rational. Yet as time goes on it

* Barlow and Pope: Cf. *Trans. Chem. Soc.*, 1906.

becomes increasingly difficult to resist the direct evidence for the simple view that, in many cases, chemical combination is not so much a fusion or intermingling of the combining atomic structures, as rather an arrangement of them alongside each other under steady cohesive affinity, the properties of each being somewhat modified, though not essentially, by the attachment of the others ; and that the space formulae of chemistry have therefore more than analogical significance. The many instances, thermal capacity, refractive index, etc., in which the physical properties of the compound molecule can be calculated additively with tolerable approximation from those of its constituent atoms, are difficult to explain otherwise. The crystallographic evidence has already been referred to.

The Spectrum.

Yet the spectrum, which the physicist is accustomed to regard as the most complete (though largely undeciphered) index of the structure of the molecule, is totally different, at any rate in the simpler combinations as compared with single atoms, unlimited groups of lines (forming bands) taking the place for the molecule of the single lines of atomic spectra. It may be permissible to believe —it is now in fact widely accepted—that no stimulation of an atom, less violent than complete disruption of some molecule in which it exists, can suffice to excite sensibly its atomic line spectrum. But there seems to be more correspondence between the absorption spectra of complex molecules and those of the molecules or radicles of which they are built up. The difference is fundamental between the firm, almost unalterable structures which are the atoms, and the molecules, considered as intimate definite aggregations of atoms capable of definite disruption ; it ought

perhaps to involve the corollary that sensible internal vibratory disturbance in the former is far more difficult to excite than in the latter. In the case of molecules of easily condensable gases, between which, therefore, strong mutual forces come into play, there seems to be evidence that the natural thermal collisions alone can excite selective radiation, as indicated by the appearance of band spectra under conditions of high temperature without chemical or electric action: in the case of atomic line spectra the negative results of Pringsheim and other observers seem consonant with what was to be expected. Line spectra are of very great luminous intensity compared with any natural continuous spectrum on which they can be superposed, unless the temperature of the latter is extremely high, and therefore the molecular collisions very violent. This seems to afford sufficient reason why they cannot be considered as in any kind of energy equilibrium with the surrounding continuous radiations.

An adequate interpretation of the master clue to dynamical molecular structure afforded by the spectrum is still lacking. The researches of Liveing and Dewar, Balmer, Rydberg, Kayser and Runge, Rayleigh, Schuster, and others, have led to the division of the simpler line spectra into correlated series of lines, with the successive vibration frequencies in each series, after the first one or two, determined by simple approximate formulæ, obviously the asymptotic forms of more complex exact relations which remain to be discovered; but very little progress has yet been made towards the dynamical interpretation of this ordered system. The radiations from electrons involve their accelerations, while those from ordinary material vibrators, as, for example, in the case of sound waves, depend only on velocities; thus, as Lord Rayleigh has remarked,

it is hardly surprising that the law connecting the overtones (so to speak) with the fundamental in each spectral series is of a type that is not met with in ordinary dynamics. A probably easier problem, as yet unravelled, is the mode of genesis of banded spectra; here the law connecting the frequencies of the series of lines which constitute a band is of type not unfamiliar,* but the known conditions in which these relations occur seem rather complex for an ordinary molecule. The facts that increased density of the surrounding medium does not shift the bands, and that the Zeeman magnetic effect is absent in bands, are very pertinent to the problem: it has been thought that these facts are somehow correlated: it may well be that the former indicates close concentration of the steady aethereal vibration into the interatomic spaces in the molecule.†

But though the problem of the dynamics of the spectrum has not hitherto yielded much under the accumulation of knowledge, the primal property of the spectrum as an analytical agent remains unimpaired. It is still true that the occurrence of a definite line marks the presence of a definite substance. With variation of the conditions of excitation that substance may or may not emit the line in question: but wherever the line is seen the inference backward is still valid, though high dispersion may of course be requisite to distinguish it from closely adjacent lines. The inference as to the presence of a substance is easy; but it would be far more difficult to establish its absence.

Among the remarkable results of recent research in this field are those of R. W. Wood on the fluorescent spectrum of sodium vapour, and the conditions of its

* Cf. "Ency. Brit.," ed. ix. supplement (1900), Art. 'Radiation.'

† Cf. *Astrophysical Journal*, 1907, p. 120.

stimulation,—for instance, the fact that excitation by a homogeneous vibration of period close to one of the free periods of the vapour excites the emission of the line of that period, accompanied by a series of lines, equidistant in frequency, ranged on both sides of it. The similarity to the circumstances of G. W. Hill's (and Adams') dynamical analysis of the lunar perturbations, first transferred to problems in other branches of physics by Lord Rayleigh, has been remarked more than once.* On the general view that a molecule is a structure such as a vortex ring or an orbital system of electrons, it has intrinsic cyclic motions of its own, which must, however, be so balanced as to avoid the unlimited draining away of its constitutive energy by radiation ; we should then anticipate that each of these structural cyclic periods would interact with the periodic disturbance exciting a natural vibration, and give rise to the analogues of summation and difference tones, if we may use the acoustic terminology. On this view the common difference of the vibration frequencies, in one of Wood's series, would be the frequency of an intrinsic cyclic motion of the kinetic system which constitutes the molecule. Here again Wood has found that many of the lines which exhibit these phenomena are specially sensitive to magnetic influence.

Recent investigation seems still to confirm that in a given spectrum the lines that shine out brightly are determined by the temperature, without concomitant chemical change : lines which appear conspicuously at one range of temperatures do not show sensibly at another, where others of the same system show up instead. We are thus required to imagine a way in which the mode of electric or other excitation of the same molecule may be different at different temperatures, just

* E.g., A. Stephenson, *Phil. Mag.*, July, 1907, p. 115.

as among the tones that belong to a bell the particular ones that ring out from it depend on the mode in which it is struck. The increased translatory motion at higher temperatures can hardly make a difference in the vibration; there remains the increased spinning motion accompanying it, which at first sight offers some promise. It might even be asked whether, in the dissociative equilibrium which arises in some gases at high temperatures, it may not be just as likely that the atoms should slip apart gradually as the result of the increased whirling motion of the molecule at the higher temperature, as that they are broken apart at the time of collision. But if ultimate separation can thus gradually arise, the earlier stage of merely modified configuration would change the periods of the spectral lines or bands of the molecules in which it occurs, and if there were enough of them, it would show as a widening of the lines of the spectrum. Thus, whatever may be the case for the far smaller numbers of degrading radio-active molecules, for ordinary gases no way is open in this direction: the change must be abrupt, and the manner in which the molecule is struck, the nature of the collision, must somehow supply the cause of the variation in the intensity of the lines. Or it may be that the dissociation which accompanies the production of a line-spectrum takes place in successive transient stages (cf. p. 30) and that the durations of these stages have an influence on the relative brightness of the lines.

Electric Phenomena.

A survey of the general features of the atomic theory would be far from adequate which omitted the fundamental atomic properties announced by Faraday in 1834 in following the path opened up by Davy's electrochemical work, and carefully formulated by him under

the new terminology of electric ions. It must have presented itself from the first to the mind of any atomist who was daring enough, in defiance of Faraday's own express caution, to transcend the limits of experimental observation, that here we have to do somehow with electric atoms, and that the essence of chemical change is involved in the passage of these entities across from molecule to molecule. Maxwell contemplated but shrank from making this plunge, being in fact fully occupied in a more accessible and equally fundamental subject, the mode of transmission and propagation of electrical influence; it was Helmholtz, further removed and thus not so much under Faraday's direct influence, who in his Faraday lecture to the Chemical Society of London in 1881* and elsewhere, first marshalled systematically the evidence that electricity must be atomic, and that the main energies of chemical affinity are, as Faraday held, of electric type. More recently the electron, or electric atom, has become a necessary idea for electrodynamic theory, if that is to include the origin of electric disturbance as well as its mode of propagation. At first the view was natural that the electron could be transferred only during the intimate encounter of molecules, and so could hardly exist free: though Helmholtz had enforced the idea that electric excitation by friction consisted in the rending of the molecules into material ions by the mutual forces arising on the intimate contact between dissimilar substances thereby produced, and later investigations have been concerned with traces of the same kind of phenomenon appearing in chemical reactions such as ionisation of air by phosphorus. The great experimental discoveries of

* "On the modern development of Faraday's conception of Electricity," "Wissen. Abhandl.," ii., pp. 52-87. Cf. Faraday, "Experimental Researches in Electricity," vol. i., No. 852, 871.

the last dozen years (J. J. Thomson, Schuster, etc.) have, however, shown that in rarefied gases, where the molecules are far too widely separated for any appreciable amount of immediate interchange, disruptive electric discharge establishes itself by driving the electron into the open, where its intense and extended field of energy has made it far more amenable to physical scrutiny than the more self-contained neutral material molecules could ever themselves have been. The enormous speed with which it travels, approximating often towards that of radiation, which is the maximum conceivable in an aether not subject to rupture, can perhaps be ascribed only to a comparable orbital velocity when the electron is in the molecule, from which it occasionally glides away through some kind of overbalancing of the internal kinetic adjustments, either enforced by disruptive electric excitation or spontaneous as in the case of radio-active substances. It appears significant that in the atomic disintegration involved in the later case the emission is at much greater speed, almost up to the limit of what is possible.

Considerations of this kind concur with the very precise magnetic subdivision of the lines of the spectrum discovered by Zeeman and Lorentz. Since Maxwell's co-ordinating analysis, we are certain that light and radiation generally are phenomena of electric disturbance: or more precisely, as he put it, we know that electrostatic phenomena are manifestations of strain, and electromagnetic phenomena are manifestations of interaction of strain and inertia, in the same medium whose undulations constitute radiation. As soon as a definite structural picture could be formed for an atom of electricity as a region of strain abutting on a central nucleus at which it is locked together, even though the free mobility of the nucleus has to be assumed rather

than understood, the suggestion of an electric theory of matter on the analogy of the vortex illustration lay open to dynamical development. Such a picture must, on principles clear ever since the time of Ampère, give positive and negative electrons, which differ essentially only as a system differs from its mirror-image, or as a right hand differs from a left hand: and the suggestion was obvious, to pass from the Daltonian principle of the identity of all atoms of the same substance, to the hypothesis of the equality of all electrons, except as regards this distinction into positive and negative. We are thus invited to discuss how far progress is possible towards a mode of representation of physical nature on this foundation alone. It will help towards wider synthesis; but we know in advance that it will hardly avail us further than the interactions between molecules across intervening aether.

The early extension of Dalton's principles which arose from Gay Lussac's laws of multiple combining volumes in gases, had already proceeded in this direction. To Avogadro the Daltonian atom itself appeared as a more ultimate constituent in the molecule, which is the actually subsisting discrete element of matter.*

Limitations

The fundamental limitation of any conceivable atomic

* The working of the presumption in favour of the simplicity of nature is illustrated by the relations of Avogadro to Dalton. For want of the simplifying idea of the molecule Dalton was logically compelled to reject the view, afterwards associated with Avogadro, that all gases contain the same number of particles: but he fell back on the next simplest hypothesis, by asserting that the particles of all gases are accompanied by the same amount of caloric in their 'heat atmospheres.' On the other hand, Avogadro demurred to this hypothesis, as really irrelevant to the particular question at issue. Yet Dalton's instinct for simplicity proved to be right, though the repelling heat atmospheres of the period were wrong.

theory, which was emphasised by Maxwell in the seventies,* seems to have lost none of its force in the years which have since elapsed. The transmission of qualities from ancestors to descendants in organic nature, now studied under the name of heredity, is of far too subtle a character to be managed by aggregations composed of discrete atoms of matter as large as we know the Daltonian atoms to be. The identity of the atoms is a safe foundation in chemistry and physics; but if an attempt is made to carry it over into biology we get lost in two directions, both in the enormous complexity of the organic chemical molecule, and in the infinite delicacy of transmission of characteristics in organic life, the latter being in sharp contrast with the rapidity of the spread of its lower forms, once inoculation has taken place under suitable conditions as to food. We are entitled to conclude, not that there is here any essential contradiction with the atomic theory, but rather that the complexities of the phenomena transcend our powers of mental analysis. Will they always do so? Every new physico-chemical explanation in physiology is fresh evidence that the processes, so far as they can be extricated, are all rational; wherever the complex of phenomena can be partially disentangled, we find order. The only method of progress is to seize the salient or large-scale manifestations of order as they present themselves, and by correlating them and fitting on new regularities thereby discovered, to go on improving the working scheme of representation. The model is not erroneous, because it is incomplete; explanation is usually worth more than criticism: an imperfect physical representation which has stood the test of substantial prediction can be improved, but it is not often absolutely refuted. Whatever be the world of reality behind and

* "Encyc. Brit." Art. 'Atom'; 'Collected Papers,' vol. ii., p. 461.

within the Daltonian atoms, of which at present we can form no idea, we are entitled by all experience to push the consequences of their physical interaction as far as possible, without fear of meeting irresolvable contradiction.

This digression, and the related postulate that the simplest representation of nature is the most probable and effective one, receive illustration from the outstanding puzzle of the electron-theory, already referred to. Why do positive electrons not exist? Put in this crude way the question is absurd. They do exist, and they are equal quantitatively to the negative electrons; otherwise ordinary molecules would not be neutral. But why are they apparently so different in structure, whereas we should expect them to be mirror images of the negative ones? The phenomena in this domain recall the other question, why have the albumens and most other vital products evolved on this planet left-handed optical structure? It may be that the union of positive with negative electricity is something much more intimate than mere orbital motion; for the structure of the nuclei of electrons is as yet totally unconceived. It may thus be that in the one or more types of molecule that happen to be practically amenable to the expulsion of an electron into the open, it is the negative that can be readily driven out. If the central nucleus which knots together the field of force constituting a positive electron were large, such as, *e.g.*, might be represented by a sphere of electrification of atomic dimensions, the intensity of its field of strain would be small throughout, its energy and inertia would thus be slight, and its effects would not be prominent except as a mere centre of attraction. All electric inertia would then belong to the negative electrons. Is there any other inertia belonging to the

atom besides electric inertia? This is the same as to ask, Is the atom something foreign to the aether and self-existent, which, however, can attract and hold certain aethereal nuclei called electrons, constituting a connexion with the aether through which atoms, which would otherwise be isolated worlds, are in physical relation with one another to form a *cosmos?* Such an extension of our conceptions may as we have seen be welcome in biological science, involving as it would that atoms in intimate entanglement may interact differently from the simple and regular methods which obtain when the aether is their only mode of inter-communication. But all who have appreciated the course of evolution of the principles of modern physical explanation through Newton, Lagrange, Young, Fresnel, Faraday, Stokes, Kelvin, Maxwell, Helmholtz, Hertz, to mention only a few names of the past, will still hold that even if the atom itself is intrinsically unfathomed, yet the interaction between atoms separated in space is in its larger features understood and has its seat in their sub-electric connexions with the aethereal medium.

Chemical Reaction in Gases.

The laws of chemical equilibrium in the rarefied state of matter, with free molecules, which belongs to gases, ought to be the most amenable to the direct indications of molecular theory: but in practice it is here that complexities show most prominently.

The earliest and one of the most complete numerical discussions of the facts of the simpler phenomena, those of gaseous dissociation, was made by Willard Gibbs himself in 1879.* In the four cases treated he satisfied himself

* 'On the vapour densities of peroxide of nitrogen, formic acid, acetic acid, and perchloride of phosphorus.' *Silliman's Journal*, (3) vol. 17: "Scientific Papers," vol. 1, pp. 372—403.

that the observations of pressure were consistent, on his own principles, with an equilibrium involving simple binary dissociation.

But subsequent detailed investigations of gaseous reactions, mainly by van't Hoff and his school, showed that often, especially when more than two molecules were concerned in the change, the results were abnormal both as regards speed of reaction and ultimate equilibrium,—and in fact indicated that the main part of the reaction occurred not in the volume of the gaseous mixture, but in contact with the walls of the containing vessel.

A discussion of these difficulties in detail is hardly to be ventured in this place, where so much of our knowledge as to the circumstances promoting or inhibiting reaction in pure gases has been acquired. But just for that reason it appears desirable to embrace the opportunity to refer to a principle, which so far as my reading goes, seems to have escaped attention.* In my copy of the English edition of van't Hoff's "Studies in Chemical Dynamics" (1896) I find the following memorandum of ten years ago. "It would be a corollary from this [estimate of chances of encounter of molecules] that in gaseous

* I find that Prof. H. B. Dixon has reasoned from the extreme rarity of trimolecular encounters, in a paper on "The Mode of Formation of Carbonic Acid in the Burning of Carbon Compounds," *Trans. Chem. Soc.*, 1896 (*cf.* p. 777), which also brings to a focus many of the peculiarities of gaseous reaction. In the same paper it was announced that the bimolecular reaction of CO with N_2O is not excited by the electric spark when the gases are well dried, though the mixture becomes explosive on the addition of water vapour. The argument in the text need not imply that every bimolecular reaction proceeds spontaneously; in such a case as the one quoted the ordinary explanation is appropriate, which regards a catalyst as opening a path of transformation around an obstructing ridge on the surface of available energy, such as would present a barrier to direct combination. As in electric phenomena, so in chemical change, the apparent anomalies of reaction in pure gases seem destined to provide the clue towards deeper insight, as, in fact, Lord Rayleigh pointed out long ago.

reactions where more than two molecules are concerned, such as $2H_2+O_2$ and $2CO+O_2$ the chance of them all being in contiguity at the same instant is extremely small compared with that for two, and the reaction can therefore only proceed very slowly, unless it proceeds by intermediate binary reactions such as the formation of H_2O_2 in the first case or reaction with contained water vapour in the second case. This superior velocity of binary combination has possibly a bearing on the specific catalytic action of traces of certain foreign vapours in facilitating explosive combination, as determined by Dixon." I see no reason to abandon the conclusion thus expressed. It is quite sound to reason, in the statistical manner introduced by Guldberg and Waage, that the relative number of direct diad combinations between molecules of types A and B is $k_{AB}.n_A.n_B$, where the factors of type n are the respective numbers of these molecules, and that the number of direct triad combinations between types A, B and C is $k_{ABC}.n_A.n_B.n_C$; but it would appear that the coefficient k_{ABC} of the latter combination must be almost infinitesimally small compared with k_{AB}. Thus if we imagine the scale of magnitude of a gas at a pressure of one atmosphere to be magnified so that the diameter of each moving molecule becomes about one inch, there will be in the model roughly about one molecule in each cubic foot, and a molecule will have to travel about a hundred feet before it encounters another one. Such binary encounters will thus happen with some frequency, and from some of them combination may ensue. But the chance of three molecules coming together simultaneously is negligible: the only way in which a trimolecular combination can arise is by one of the molecules attaching to itself another, in a manner perhaps relatively transient, and this pair going off together to meet the third, each acting so to speak as a

carrier for the one united with it. Without some such intermediate transient stage of combination a dissociation of a complex into three molecules must proceed to an end, for the chance of an equilibrium through recombination would be negligible. Where an equilibrium is found to become established, either the reaction must occur in binary stages, or else it must take place in contact with solid or liquid boundaries where the molecules form a denser layer in which each is always in relation with others. This consideration reinforces the importance of the study of reaction in pure gases, as a means of disentangling the intermediate stages of chemical combination and the durations of the products formed in them. The fundamental importance of this kind of knowledge for the adequate interpretation of banded spectra has already been alluded to. It appears, indeed, to be commonly recognised that direct trimolecular combinations occur seldom: the inference from the present line of argument is that in gaseous reactions they do not occur at all. Recently I have learned that Mendeléef had always maintained that gaseous reaction occurs in monomolecular or bimolecular stages in all cases: there seems to be strong presumption in favour of such a view.

It would require the instincts of a chemist to venture on any attempt to apply this principle to special cases, to discuss why, for example, the presence of a foreign substance sometimes promotes the occurrence of the necessary intermediate binary reaction, and in other cases presumably destroys its product, and so inhibits the final transformation. The recent results obtained by Bone and Edmunds for the thermal dissociation of H_2O agree with the conclusions drawn in 1884 by Dixon with regard to the explosion of a mixture of CO and O_2, in assigning

the presence of hydrogen free or combined as a potent stimulus to ternary reaction.*

Ionisation and Solution: Available Energy and Berthelot's rule.

At first sight it might appear that the principles of statistical equilibrium in dilute systems would afford a criterion as to the intimate process of dissociation, whether into ions or molecules. Thus, to fix the ideas, the two reactions, $HCl = H + Cl$ and $2HCl = H_2 + Cl_2$ would come to different equilibria, of the types $n = k \cdot n_1 n_2$ and $n^2 = k' \cdot n'_1 n'_2$. But in reality the subsequent aggregation of H and H into H_2 itself involves an equilibrium $n_1 = \sqrt{(\kappa n'_1)}$, so that the discrimination is not possible on these considerations alone. Nor is it ever possible in this way on the thermodynamic theory, which can be seen (*cf.* Appendix) to be consistent with separate independent equilibria as regards every type of reaction that is formally possible in the system.

The process of ionisation in a liquid solvent is obviously very different from ordinary gaseous dissociation. The view that some such special type of dissociation is required in order to form a coherent mental picture of Faraday's electrolytic results, must really in strict logic go back to Clausius' ideas of about fifty years ago. It is true that he did not venture to suggest more than extremely slight ionic dissociation. But once the mere possibility is granted, there is no ultimate escape from the permanent ionic separation of Arrhenius: for it is only a question of making the solution more and more dilute in order to diminish indefinitely the chance of

* I have ventured to add an abstract discussion on the formal possibilities that are open, which was drawn up about a year ago, as an Appendix.

any two ions ever meeting again to unite, as compared with the unaltered chance of any remaining whole molecules becoming divided into ions. Complete ionisation must ultimately arrive; and there is only the question remaining over as to the degree of dilution at which it is practically attained.

It will be observed that we are in this argument applying the principles of mobile equilibrium to the ionisation of the dissolved substance. Here an appeal to experiment becomes feasible. It was by Ostwald that this test of Arrhenius' view was first applied. As is well known, he found that for acids and salts that are but slightly ionised, *e.g.*, acetic acid, the mathematical relation expressive of equilibrium of simple dissociation is satisfied; but for highly ionised substances it is widely departed from. The verification in the former case appears to be sufficient by itself to confirm the general point of view. For it seems natural to suppose that high ionisation indicates the presence of some type of direct affinity with the solvent, which is too powerful to be altogether omitted from the equation which expresses the ultimate equilibrium. How such an influence should be included is one of the main unsolved problems in this subject. At higher concentrations either the tendency to re-combination of the ions is resisted, or else the tendency to ionisation of the molecules is promoted. Thus if increased ionisation were a result of collision of molecules with the existing ions, in analogy with known effects of collision of (rapidly moving) ions with the molecules of gases in promoting further ionisation, the equation of equilibrium would be altered from $n_1^2 = kn$ to $n_1^2 = kn + k'nn_1$, in which k'/k would be sensible only for easily ionisable substances (cf. Appendix). The solution of this problem, if there is any simple colligatory

principle, must, however, be a matter of experimental scrutiny.

The acceptance of the idea of ionic dissociation by solution has been opposed by scruples of a more fundamental kind, not altogether unlike the difficulties once attaching to the Berthelot doctrine that the extent to which a reaction proceeds depends on the relative amounts of reacting substance. It is a fundamental postulate that a molecule is a self-existent aggregate, whose intrinsic binding affinities are independent of temperature: as we have seen, one of the main *à posteriori* reasons for this conclusion is that each (sharp) line of the spectrum is characteristic of the molecule, alterable in position only very slightly, or not at all, by any change of physical conditions. And this view agrees with the kinetic theory which connects temperature with the average translatory (and concomitant rotatory) motions of the molecules in space, and sometimes with partial dissociation, but not with any intrinsic change in structure in the molecules that are present. Thus the bonds of atomic affinity which have to be overcome, say in the ordinary non-ionic dissociation of a gas, are the same at high temperatures as at low. But occasionally a collision with another molecule may be well-directed towards breaking these affinities, like the sharp impact of a mason's trowel on a brick or tile, and as a rule it will be the more effective the higher the temperature. The verification of the theoretical law of equilibrium, in ordinary gaseous dissociation, enables us to assert that strong affinities are in fact occasionally thus shattered; that high affinity is to be measured not by entire absence of dissociation but by its relative rarity, though the products of disruption can be accumulated when the opportunities for recombination are removed. The dissociation into ions

in a solvent is perhaps a more fundamental change than the ordinary dissociation of a gas: yet the ions may accumulate notwithstanding, for the obstacles to recombination presented by the dense molecular aggregation of the liquid in which they are entangled are also enormous in comparison with any that are present in the gas. Thus the circumstance that self-ionisation is hardly detectable in a gas is not conclusive evidence that it never occurs without the assistance of a liquid solvent medium.

It seems worth while to follow up these relations somewhat further in the light of Faraday's conclusion, so emphatically enforced by Helmholtz in 1881, that the strongest forces of chemical affinity are of electric type. It would almost seem as if we must adopt the view that the active atom in ordinary chemical change is the ion, with its large intrinsic electric charge as an essential feature. No permanent state such as we associate with a simple dense material substance can be reached until these enormously active positive and negative bodies have become paired; until, in fact, their domains of activity, in place of being the widely ramifying fields of force of free ions, are changed to the more concentrated and individualised fields of molecules,* in which the lines of force instead of spreading out far into space simply pass across in more or less curved lines from one ion to its adjacent conjugate. No substance could exist completely ionised in free space for a moment, nor with any considerable excess of ions of one sign: ionisation is, however, continually occurring in substances to a small extent, spontaneously and so to speak by accident, *i.e.*, in a manner not con-

* In the molecular groups of solvent media, which have abnormally high dielectric capacities, the two conjugate ionic poles are so far apart that these groups may be held to occupy a position intermediate between ordinary molecules and active ions. Cf. p. 37.

trollable, the duration of separate existence of the ions depending on the obstacles to their finding new partners In the case of gases in which the formation of ions is stimulated by electric shock (Röntgen rays, etc.), knowledge is now highly developed (J. J. Thomson, Townsend etc.) regarding their rate of formation, and its equilibrium with recombination or with their extraction from the region by an electric field: the processes in solutions are much more rapid, and only resulting equilibria can be directly investigated. The operations of inorganic chemistry consist largely in presenting to these ions of solutions the possibility of taking on new partners, either by simple admixture, or by pulling them away into a new environment by electric agency. The energy required for the guidance of chemical processes is expended in this latter way, perhaps none of it goes (except very indirectly through thermal interchange) to the pulling asunder of the ions in the atom; that separation takes place sporadically, with purely local adjustment of energy, to an extent dependent, however, on the environment and in this way modifiable.

Any intense motional disturbance liberated in such ionisation lapses into mere thermal energy. But by means of guiding control (constraints in bulk) little of it may be left to this fate—*e.g.* in the Daniell voltaic cell, as Lord Kelvin discovered half a century ago. Here again we recall the emphasis placed by Helmholtz, in 1881, on the inference that in a voltaic cell there is but slight expenditure of energy in getting the current across the solution, merely, in fact, the Joulean heat, while the large amounts of energy which become available or stored have their origin at the electrodes, in that process (often, as will be seen, nearly statical) of passing over the Faraday unitary electric charge from atom to atom, which is,

therefore, the essence of the change in the state of chemical combination.

The source of the energy required for the natural degree of ionisation which occurs when, say, K.HO is dissolved in water, has been a standing problem in this department. The current mode of explanation seems so far sound, that when a dilute base is neutralised by an acid, say K.HO by H.Cl, the resulting heat is derived from the free ions H and HO clashing together at high speed generated by their strong electric attraction, as H_2O does not exist sensibly ionised—that therefore the heat evolved is about the same per chemical equivalent in all such cases. But when K.HO is added to water, whence comes the supply of energy demanded for the pulling apart of the K and the HO, into separated ions, against their mutual attraction? The concomitant absorption of heat is far too slight to account for it.* We are tempted to conclude that internal potential energy is released owing to the ions falling into relations of closer affinity with the solvent, and that the process is nearly a self-contained interchange of energy, reversible as regards each molecule separately, being a steady static drawing apart of the ions unaccompanied by the generation of violent subsidiary electronic motions whose energy would escape into the general store of heat. The fact above alluded to, that in voltaic batteries so large a proportion of the chemical energy is usually mechanically available, also seems to point this way; it shows, too, that the fundamental interchanges of electrons at the electrodes, which are the sources of the transformation of energy, are intrinsically of the same not merely reversible but almost

* *Cf.* G. F. FitzGerald: Helmholtz Memorial Lecture, *Trans. Chem. Soc.*, 1896: "Scientific Writings," p. 363 *seq.*, also p. 521.

static type, accompanied by but little energy of intense agitation such as would be partially dissipated into the surroundings. It may thus be the close quarters at which these operations are developed, in the liquid environment, that limit both the occurrence and the diffusion of irregular intense disturbance such as would pass away into sensible heat. In ordinary dissociation of the nearly free molecules of gases, accompanied by comparatively large changes of volume or pressure, or in the cognate phenomena involving osmotic expansion of dilute solutions already formed, a relatively greater degradation of energy is involved, and is indicated by the greater variation of the equilibrium on change of the temperature.

The history of this problem may be recalled. When Lord Kelvin opened up the subject of availability of chemical energy in 1851, he found by experiment that in a Daniell cell nearly the whole of the energy of chemical combination was available for mechanical work. Later Gibbs in 1878 and Helmholtz in 1882 pointed out that the change of electromotive force with temperature gave a measure of the proportion of the energy that is not thus available: and the accumulating cases of discrepancy with Kelvin's principle thus became rational. Now the problem is rather why the unavailable part proves to be often so slight, as compared with other chemical processes more thermal in character. Not merely can the operations be conducted in a nearly reversible manner, so that all the available energy is utilized, but in addition nearly all the energy of the chemical change is often actually available so that there is but slight evolution of heat where it occurs. It will be remembered that Professor Nernst devoted his recent Silliman Lectures to this subject. "To enable us to proceed it is necessary to find the conditions under which the principle of Berthelot comes

nearest to expressing the true relation between [available] chemical energy and heat, or, what amounts to the same, between the magnitudes A and Q. In this direction we can show that in reactions between solids, liquids, or concentrated solutions, the values of A and Q approach each other very closely, while on the other hand in dilute solutions or with gases we actually find large differences between the two quantities; . . . "* The case of galvanic cells operating even by dilute solutions is included in the generalization for the reason given above. Obviously also on the present view the unavailable part of the energy should become steadily less at lower temperatures, as Nernst concludes. The principle of Gibbs, that the fraction of the energy of chemical combination that is unavailable is equal to the ratio of the actual temperature of reaction to the temperature of dissociation, provided correction can be made for work of expansion and heat on change of state, etc., is seldom effective on account of this latter complication.

The Faraday unitary charges have now a specific name, the electrons. Their unchanging magnitudes were strong presumption from the first of their intrinsic atomic existence: the Zeeman-Lorentz effect has almost exhibited them to us in action in the molecule, as the agents of radiation through their combined vibratory motions, in the now familiar manner foreshadowed by the Maxwell-Hertz theory of radiation. But the most far-reaching of recent discoveries has been that not merely can they pass at close quarters from molecule to molecule in some hitherto inscrutable way, according to the Faraday law, and also reveal their vibrations inside the molecule through its spectrum and its magnetic modifications,

* Nernst, *loc. cit.* "Applications of Thermodynamics to Chemistry," 1907, p. 43, *seq.*, where extensive examples are given.

but that they can be drawn out into the open by electric shock, and securely manipulated (J. J. Thomson, Lenard, etc.) as atoms of pure disembodied electricity in those cathode streams across highly rarefied gases which Sir W. Crookes long ago insisted on calling a fourth state of matter.

In the electrolysis of an acid we are to imagine the negatively charged massive hydrogen ions, which happen sporadically to be free in the solution, as being slowly drawn towards the negative electrode by the electric field pervading the medium, as accumulating there with production of polarisation reacting against this impressed electric field, until they are so crowded together by the constraint that some kind of instability arises, whereby one of them takes over, but without violent disturbance such as could diffuse away, two positive unitary charges from the electrode. Surely these charges must in ultimate analysis be positive electrons, or else the power of losing negative ones must be unlimited. At any rate, in virtue of them, the ion can associate with another and become released as a free self-contained molecule of hydrogen, a very different thing from the mutually constrained ions that gave rise to it,—the energy required for the electrolysis being expended mainly, as Helmholtz insisted, and, as we have seen, usually without much necessary waste, in this requisition of the two positive charges.

The relevance of the mode of operation of the Grove gas battery will here occur to mind; the finely divided or porous platinum surface promotes ionisation of the gas alongside it through the opportunities arising from intimate contact in confined spaces (*cf.* p. 37), and so manages to utilize much of the available energy of gaseous combination, which in gases is very different from the heat of combination on account of the change occurring

in the energy of expansion. Fortunately for exact knowledge, the principles of thermodynamics give in all such cases the means of estimating the final result, without requiring hypothesis as to the nature of the process that is involved, provided only it be reversible; in dilute systems the argument can be expressed in terms of the available energies, or thermodynamic potentials, of the constituents, interpretable in the simpler cases by partial osmotic pressures—in extension of which the idea of solution pressure of an ion can be employed as a graphic mode of expression, without implying that the processes involved can be placed in effective analogy with those of evaporation or solution. The very remarkable quantitative connexion of the electric potential gradient between solutions with the diffusions of their ions, established by Nernst, must ever remain one of the solid foundations in this subject.

The modern expression of Helmholtz's provisional generalisation, that chemical affinity arises from intrinsic attraction of the different kinds of matter for electricity, may roughly be that every active atom is an ion, and that saturated inert molecules are welded into unity by each constituent atom keeping hold, through the aethereal agency of electric attraction, of the (perhaps interpenetrating) electrons belonging to the other. The electrical view provides a reason for the ordinary saturated inactive molecule of elementary bodies being often polyatomic, a fact otherwise of undiscovered import. The exceptions afforded by monatomic gases and metals indeed suggest themselves at once: but spectrum analysis shows that these molecules are intrinsically just as complex in sub-electric structure as the others, while the physical test of monatomicity perhaps only verifies that the components of the molecule are somehow so closely

compacted that the thermal collisions do not induce sensible internal commotion.

But it is time to conclude this discursive survey. We have recognised that the Daltonian molecular theory is still the indispensable guide, if we wish to continue constructive efforts in the physical elucidation of nature, and are not content to take down our scaffoldings for the sake of logical symmetry, and, in the future, make the most of the edifice as it now stands. While we are certain with Dalton that molecules are very definite, identical, structures, it has been seen that, when we inquire into the detail of their constitution, though many guiding principles mainly of electric and spectroscopic types have been made secure, yet we have not much more than their distant analogy with familiar dynamical systems to aid us. But for many branches of the science knowledge of detailed molecular structure is not required. The pioneering example of this kind was the kinetic theory of gases. The domain of electrodynamics is now securely founded on the displacements and movements of electrons, each of which may be considered merely as a point at which the unitary electric charge is concentrated, so small are the unknown nuclei of the electrons compared with their distances apart. In the same way the wide domain including the course and equilibrium of reactions in dilute systems can be studied by pure numerical statistics in the manner of Guldberg and Waage, or by the more generalised but fundamentally equivalent thermodynamic methods associated mainly with Willard Gibbs. But the aim of structural chemistry must go much deeper; and we have found it difficult, on the physical evidence, to gainsay the conclusion that the molecular architecture represented by stereochemical

formulæ has a significance which passes beyond merely analogical representation, and that our dynamical views must so far as possible be adapted to it. We have recognised that the interaction of atoms at a distance apart, which is necessary to a *cosmos*, is provided for by a very special mechanism, consisting in the activity through the aether of the electrons that are attached to them. The artificial aspect of this arrangement would be relieved if we could assume these entities to be of the essence of atomic structure; we are justified in following out this hypothesis as far as it can carry us; and the totally unexpected phenomena of disintegration of complex atoms, very definitely detected, even in part predicted, by Rutherford and his colleagues and successors (Soddy, Ramsay, etc.), itself arising from Becquerel's and the Curies' discovery of spontaneous radio-activity, may ultimately lead us far. But there remains the question whether the facts of biology demand an underlying complexity in the atoms vaster than could be embraced in any definite physical scheme.

Our conviction of an orderly connexion between things constitutes the conception of a *cosmos*. We have placed the foundation of this in the existence of a uniform medium, the aether, the physical groundwork of interstellar space, through which the actions between material bodies are established and transmitted. The idea of such a medium, when analysed mathematically, almost demands that matter should consist of discrete atoms, involving nuclei each of which binds together into permanence some mode of local disturbance in the medium. The illimitable complexity of the phenomena resides in matter; but our grasp of the physical relations to which all its manifestations are subject arises from their being such as can be established through the aether. The

Daltonian principle of identity of all atoms of the same substance,—they are the same to the remotest limits of our universe, as Huggins demonstrated—may well arise from these atoms being the limited number of definite intimate types of structure into which more ultimate atoms can arrange themselves. These ultimate atoms would be limited as regards their relations at a distance, for they would in this respect involve only the few fundamental types of strain-centres which are capable of subsisting in the simple aether. The keystone of such a physical scheme is the aether: and the only ground for postulating the presence of this medium is the extreme simplicity and uniformity of the constitution which suffices for its functions. Needless to say, there remain many unresolved features, some still obscure, but hardly contradictory. But should it ever prove to be necessary to assign to the aether as complex a structure as matter is known to possess, then it might as well be abolished from our scheme of thought altogether. We would then fall back on simple phenomenalism; proximate relations would be traced, but we need not any longer oppress our thought by any regard to a common setting for them; the various branches of physical science would cultivate with empirical success independent modes of explanation of their own, checked only by the mutual conservation of the available energy, while the springs of their orderly connexion would be out of reach. That time, however, is not yet.

APPENDIX.

*On the Possible Types of Direct Chemical Combination.**

The question must often have arisen why chemical combinations and decompositions take place on a simple plan which can be represented as the addition to the molecule, or removal from it, of definite blocks or complexes of atoms, named radicles, which are therefore assumed to have a transient corporate existence; so much so that in a class of cases their existence has to be introduced explicitly in order to assist as carriers in the reactions. The physical analogy of an atom as a kinetic system of orbitally moving sub-atoms or electrons, such as is pointed to by its very definite intrinsic spectrum, would lead us to expect that, as a rule, tampering with the structure of a molecule by slicing off a block would lead to its total dissolution: while, on the other hand, the practically effective conceptions of organic chemistry suggest architecture rather than dynamics.

Some light may perhaps be thrown on this subject by the consideration that it is only those structures that do fall to pieces in successive stages, and are thus capable of definite experimental dissection, that can have a chance of being produced in quantity, and of becoming segregated, in the clash of molecules amidst which all things chemical have their origin.

In illustration, imagine a substance, say gaseous for simplicity, formed by the immediate instantaneous combination of three gaseous components *A*, *B*, *C*. When these gases are mixed, the chances are very remote of the occurrence of the simultaneous triple encounter of an

* This Appendix was included substantially in a course of lectures at Columbia University, New York, in March, 1907.

A, a *B*, and a *C*, which would be necessary to the immediate formation of an *ABC*; whereas, if ever formed, it would be liable to the normal chance of dissociating by collisions; it would thus practically be non-existent in the statistical sense. But if an intermediate combination *AB* could exist, very transiently, though long enough to cover a considerable fraction of the mean free path of the molecules, this will readily be formed by ordinary binary encounters of *A* and *B*, and another binary encounter of *AB* with *C* will now form the triple compound *ABC* in quantity. The cognate subject of the dynamics of gas theory illustrates the point, in fact is closely implicated: that theory proceeds by aid of statistics of encounters, yet in its analysis triple and multiple encounters are left aside as negligible in number.

The principle thus suggested, that immediate molecular combinations and dissociations are practically all binary, may have a wider application. It would tend to explain, as above indicated, how it is that in organic chemistry only those types of molecules—perhaps very few compared with what are in totality possible—have any chance of being isolated amid the chances of natural reactions, involving no control whatever of individual molecules, which can of themselves divide into parts or radicles of appreciable persistence, some of them replaceable by other such blocks or parts without allowing time for the dissolution of the whole. That science, in fact, proceeds by searching out and classifying the ways in which complex molecules may be thus definitely dissected and reconstructed.

There is involved, on this view, the proposition that in the ultimate type of chemical interaction each molecule is divided into two parts at most, which it may interchange with another molecule in the process of double

decomposition; that in fact any transformation of more complex type than this must be expected to occur in successive stages. Thus the simplest case of a triple dissociation, say of a molecule ABC into A and B and C, may be held to occur in two stages, the first stage being such as a change into AB and C: the reason may be repeated, that without an intermediate diad stage the velocity of association of such molecules would be extremely slow compared with the velocity of dissociation of those already formed, so that in equilibrium the triad compound would practically not exist.*

It appears indeed from the facts that chemical equilibria involving processes more complex than double decomposition occur but rarely. Where they occur at all, it is here suggested that there must be an intermediate stage, perhaps very transient; and the question arises whether its existence may modify the usually accepted deductions from the Guldberg-Waage statistical representation of the chemical equilibrium, or the application of thermodynamics of which that theory forms one aspect.

The matter will, however, assume a sufficiently complicated form, at any rate for initial consideration, in the very simplest example. Let us then examine the chemical equilibrium of a dissociating gaseous substance ABC, mixed with its components. Let this symbol ABC denote quantity of the substance ABC, measured in chemical equivalents, say by number of molecules per

* Considerations such as these must often have occurred to molecular theorists. It is only recently (a year after this note was composed) that, in an obituary notice of Mendeléef by Dr. E. C. Edgar (*Manchester Memoirs* 51, 1907) a remark about "his persistent devotion to the Mendeléef-Gerhardt law, that gases combine only in equal volumes" has prompted a reference to the section on Atoms and Molecules in the "Principles of Chemistry," where views essentially equivalent to the above are powerfully supported on purely chemical grounds. [See footnote, p. 29 *supra*.]

unit volume, with similar meanings assigned for the other symbols. There can be present in the interaction seven substances, viz., ABC, AB, BC, CA, A, B, C, some of them perhaps of such slight permanence that they are not apparent.

Of the amount ABC, suppose the quantity $k_1 . ABC$ changes into BC and A per unit time, $k_2 . ABC$ into CA and B, $k_3 . ABC$ into AB and C: suppose the quantity $a . BC$ dissociates into B and C per unit time, and so on: suppose $f . B . C$ is the quantity which associates into BC per unit time, and so on; suppose $l . BC . A$ is the quantity which associates into ABC per unit time from BC and A, and so on :—the coefficients $a, b, c, k, \ldots$ being proper fractions. Thus we have a scheme of formally possible transformations

$$(k_1 + k_2 + k_3) ABC$$
$$a . BC,\ b . CA,\ c . AB$$
$$f . B . C,\ g . C . A,\ h . A . B$$
$$l . BC . A,\ m . CA . B,\ n . AB . C.$$

If any of these intermediate substances (say AB) is so transient as practically not to occur, the corresponding association factor (h) must be very small compared with the dissociation factor (c).

When chemical equilibrium is attained, the dissociations and associations continually going on do not alter the amount of the substance A; therefore

$$k_1 . ABC + b . AC + c . AB - g . A . C - h . A . B - l . BC . A = 0,$$

and there are two similar equations.

In the same way the constancy of the amount of the substance BC requires

$$-k_1 . ABC + a . BC - f . B . C + l . BC . A = 0,$$

and there are two similar equations.

And the constancy of the amount of the substance ABC requires

$$(k_1+k_2+k_3)ABC - l.BC.A - m.CA.B - n.AB.C = 0.$$

There are seven equations in all, but only four can be independent; for the total amount of A that is present, free and combined, cannot change, therefore

$$A+AB+AC+ABC$$

is constant, and there are two other such relations. This reduction is verified; for adding any one of the first group of equations to the corresponding one of the second group gives the same result, viz.,

$$a.BC+b.CA+c.AB=f.B.C+g.C.A+h.A.B,$$

and subtracting the sum of the first group from the last equation also gives this result.

We shall take the second group and the last equation as the independent relations. To eliminate the intermediate substances AB, BC, CA, we have from the former

$$BC=\frac{k_1.ABC+f.B.C}{a+l.A};$$

and substituting in the last equation the value of $l.BC.A$ thus derived,

$$\frac{lf.A.B.C-ak_1.ABC}{a+l.A}+\frac{mg.A.B.C-bk_2.ABC}{b+m.B}+\frac{nh.A.B.C-ck_3.ABC}{c+n.C}=0,$$

a complicated relation which, in conjunction with the expressions for binary combinations such as BC above, and the total atomic amounts of interacting material, determines the equilibrium.

If $l/a, m/b, n/c$ are very small, and f, g, h correspondingly large, so that the intermediate compounds AB, BC, CA are all very transient, we have very approximately

$$\left(\frac{lf}{a}+\frac{mg}{b}+\frac{nh}{c}\right)A.B.C=(k_1+k_2+k_3)ABC,$$

which is the type of formula usually assigned for the equilibrium of a triple dissociation. When the amounts of interacting materials are given, this formula determines their distribution.

If BC and CA are very transient compared with AB, we have l/a and m/b very small compared with n/c, while f and g are large compared with h if they take a sensible part in the equilibrium; then

$$\left(\frac{lf}{a}+\frac{mg}{b}+\frac{nh}{c+n.C}\right)A.B.C=\left(k_1+k_2+k_3\frac{c}{c+n.C}\right)ABC.$$

But if we suppose only five substances sensibly operative, say ABC, AB, A, B, C, the equations will be the (usual) binary ones,

$$c.AB=h.A.B, \qquad k.ABC=n.AB.C,$$

yielding

$$kc.ABC=hn.A.B.C,$$

which is a different law of equilibrium, being the same as if AB also did not occur.

If A and B and C are identical, this latter law will hold universally: if only A and B are identical, it need not do so. Generally, the conditions for its validity are that l, m, n should be very small, or else $l/a=m/b=n/c$.

The *thermodynamic* condition of equilibrium employs conceptions and physical constants different from those pertaining to this statistical view of Guldberg and Waage, but at bottom connected and in ordinary cases leading to the same results. If m_1, m_2, m_3, m_{12},...denote the quantities of the different simple and compound substances that are present in any phase, and A the available energy,

$$\delta A=\ldots\ldots+\mu_1\delta m_1+\mu_2\delta m_2+\ldots\ldots+\mu_{12}\delta m_{12}+\ldots\ldots$$

And as the available energy tends to a minimum, under the appropriate conditions, including constancy of tem-

perature, any slight reactive change that can occur in the phase must leave A sensibly unaltered, provided equilibrium has arrived. Thus the thermodynamic potentials

$$\mu_1, \mu_2, \mu_{12}, \cdots$$

in the phase must satisfy a number of relations indicating the equilibrium of each possible partial reaction that can occur in it, *e.g.*, as there is a reaction possible of type

$$m_1 + m_2 \rightleftarrows m_{12}$$

we must have

$$\mu_1 + \mu_2 = \mu_{12}.$$

The thermodynamic potentials of all compound substances in the phase are thus found in terms of those of the simple (or other) independent constituents

$$m_1, m_2, \ldots\ldots m_r;$$

that is, the system will settle down to an equilibrium in which they have the values thus determined.

If two phases coexist in contact, $\mu_1, \mu_2, \ldots\ldots \mu_r$, must moreover have the same values in both of them.

If P phases can coexist, there are thus $r(P-1)$ conditions to be satisfied: and each phase has a characteristic equation of state connecting m, v, and the temperature T,—thus making up in all P conditions. Now there are independent variables rP in number, together with T, and the total volume v,—the portions of the volume occupied by the various phases being determined by their characteristic equations. The system will be wholly determined if $P = r + 2$.

This is, in fact, Willard Gibbs' theory limiting the number of phases that can coexist in given material, and conversely. The thermodynamic potentials that are here involved must be functions of the velocities of interaction of the previous analysis. But are they always consistent with the previous statistical view, without restriction?

In dilute solution $\mu \propto \log(m/v)$, thus we should have a relation $m_1 m_2/m_{12} = \text{constant}$. Thus all the thermodynamic equations of equilibrium will take the form of the constancy of simple factorial ratios. Does this imply more than mere statistics of chance encounters can provide? It involves a further principle, that the reaction between m_1, m_2 and m_{12} is in equilibrium *by itself*, just as if the other components containing the same elements were prevented by constraint from changing. If this principle of isolation of the equilibria of the component reactions is warranted, it produces extensive simplification not inherent in the customary statistical point of view : it can for instance specify at once the proportions of the intermediate compounds that are present, replacing a system of complex linear equations by constancy of simple ratios.

If we may apply it to the problem on page 48, the equations there deduced will be replaced by

$$k_1 . ABC = l . BC . A, \ldots\ldots \quad f . B . C = a . BC, \ldots\ldots$$

so that

$$k_1 . ABC = \frac{lf}{a} A . B . C ;$$

involving

$$k_1 \Big/ \frac{lf}{a} = k_2 \Big/ \frac{mg}{b} = k_3 \Big/ \frac{nh}{c} ;$$

and also expressing the relative frequencies in which ABC splits up into different intermediate compounds.

In further exemplification of the simplification thus introduced, consider the system N_2O_4, NO_2, N, O ; if we are sure, experimentally, that N and O are infinitesimal in a partial system NO_2, N, O, then they are so in the wider system, for by the equilibrium of the partial system N and O are determined.

On this thermodynamic view it is in fact by trains of single or double decomposition that substances are formed.

For if we do not admit this postulate, then the equations of statistical equilibrium will contain more than two terms as exemplified above; and that aspect of chemical equilibrium will be at variance with the usual thermodynamic theory, which expresses an independent equilibrium for every type of reaction that is formally possible. And the reason has been already indicated, viz., the usual expressions for the thermodynamic entropy and available energy of gaseous systems, and through them of dilute solutions, involve the implication that only binary molecular encounters need be considered. The two points of view will agree only if all reactions take place in binary stages; and it becomes a question whether this is a universal rule under all circumstances, or only one prevalent in the prominent cases which are naturally those governed by simple recognisable relations.

An actual case in which these distinctions may make theoretically a difference is worked out from the thermodynamic side in Planck's *Thermodynamics*, §247, under the heading of graded dissociation, viz., that of hydriodic acid HI into H_2, I_2, and I.

Another question in which such considerations may have scope is that of Ostwald's law of equilibrium of ionisation. If only two ions can arise, they must be equal in number; thus if c' is their concentration (dilute) and c that of the non-ionised part, c'^2/c may be expected to be constant at each temperature, the ionisation proportional to c being balanced by the recombination proportional to c'^2. But this assumes that all the ionisation is spontaneous, whereas in the cognate phenomena of gases the encounter of an ion (in rapid motion) with a molecule has been shown by Townsend to be a potent cause of further ionisation. This suggests the question whether c should not be replaced by $c+kcc'$ or $c(1+kc')$, which may

make a difference in the direction actually occurring, whenever the concentration of ions c' is considerable. Moreover, the spheres of mutual electric influence of ions are far greater than those of molecules, which may also make a difference.

It is, however, to be noticed that in the discussion above of the ordinary association of a substance ABC, this type of action, in which *e.g.* a component C acts in a special manner in breaking up a component AB, has been excluded. The presence of such actions in which more than one cause contributes to the result would seem hard to adapt to the usual thermodynamic theory involving as we have seen independent binary stages of reaction.

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XI. Notes on the Greater Horseshoe Bat, *Rhinolophus ferrum-equinum* (Schreber), in Captivity.

By T. A. COWARD, F.Z.S.

(*Received and read February 25th, 1908*).

In the winter of 1906-7 I paid a short visit to Cheddar, Somerset, in order to study the habits of the Greater Horseshoe Bat, and brought back with me two living bats, obtained in one of the caves on January 6th, 1907, for observation at home; one of these survived a fortnight, the other five weeks. I published an account of my observations and conclusions (1), and propose to give an epitome of my paper, as it has considerable bearing upon my more recent notes.

In the Cheddar caves I found numbers of Greater Horseshoe Bats scattered singly or in colonies. They were not in profound sleep, and moved their positions from time to time; they were, in the evening, occasionally on the wing in the caves. On two or three evenings I saw bats emerge from a fissure in the roof of one cave and fly further into the cave, and on one night—January 6th, 1907—Mr. C. Oldham, who had then joined me, and I watched bats emerge from this fissure and pass out of the cave into the open. On the floors of the caves, sometimes scattered and sometimes in little heaps, were the rejected portions of insects which had been devoured by the bats. These consisted of wings and other parts of moths, evidently captured in summer, and fragments of beetles, and of a cave-spider, *Meta menardi*, Latr. The beetle remains consisted of the head, prothorax and first pair of

legs, and often with the mesothorax and elytra or one elytron attached, but the abdomen and wings had been in almost every case devoured. There was old dry dung in heaps, and more recently-dropped dung scattered about. Mr. R. Newstead examined the dung on my behalf, and found that the pellets dropped in summer contained about equal proportions of fragments of Lepidoptera and Coleoptera, and that about 44% of the coleopterous remains were those of some species of *Geotrupes*. The dung dropped in winter was almost entirely composed of remains of *Geotrupes*, and many of the rejected fragments found in the caves were of *Geotrupes spiniger*, Marsh. The elytra of a flightless beetle were also present in a few places in the caves.

"The prey of the Greater Horseshoe," I wrote, "may be captured on the wing, but that it is not, as a rule, devoured whilst the bat is flying, seems to be proved by the behaviour of bats in captivity even more than by the presence of fragments of prey in the caves. When secured by a snap of the bat's jaws the insect is conveyed to some resting-place and there consumed." Over 120 beetles—*Geotrupes typhœus*, Linn.—were eaten by my two captive bats, and in every case the behaviour of the bats was practically the same. "I usually held the bat in my hand until it had snatched the beetle and then released it;—The released bat, holding the beetle securely,—flew to some favourite foothold, and there hung until the beetle was devoured. I never heard the sound of champing jaws as the bats were flying, but when they were at rest the noise of crushing the hard armour of the beetles was plainly audible. The interfemoral membrane was never used as a pouch, as it is in the *Vespertilionidae* (2), but the beetle was invariably pushed against the interbrachial membrane, as I observed was the case in the Lesser

Horseshoe (3). As a rule one leg was detached from its hold in order to give more freedom to the half-outstretched wing on the same side."

On December 28th, 1907, I received two female Greater Horseshoe Bats from Mr. Bruce F. Cummings, which he had taken two days previously in iron-workings in the Pickwell Down sandstone at Braunton, North Devon. These I had fed upon *Geotrupes typhœus*, and succeeded in keeping one alive for five weeks and the other nine weeks. For some ten days I was obliged to feed the bats by holding them in one hand and pushing beetles against their jaws with the other, but in less than a fortnight both would take proffered beetles, snatching them eagerly from my fingers. It was perfectly easy to distinguish one bat from the other, for they differed greatly in their tameness, and the younger and more familiar of the two had a large female tick (*Ixodes vespertilionis*, C. L. Koch) on its back. This tamer bat was the first to take beetles from my fingers, to find them and devour them in its cage, and to catch them for itself on the wing. The tamer bat would frequently pitch on my hand or arm, brush in flight against my head, hang itself up close to my face, and show plainly in various ways that it wished to be fed. The other one was always wilder and more nervous, but it would, when suspended from my finger, allow me to carry it about the room, and drink when I held it over a saucer of water or devour a beetle without leaving my hand.

The size of the cage in which I kept my bats is 18 ins. by 18 ins., and 24 ins. high; a bar of wood is fixed at 20 ins. from the floor of the cage from which the bats suspend themselves. In this somewhat limited space they were able to find and catch beetles which were left in the cage. When feeding the bats in the evening I

always allowed them to fly about the room and pitch wherever they pleased.

Considerably over 400 beetles were eaten by the bats, whilst the latter were at rest. The exceptions were so few that I have no hesitation in asserting that it is the rule for the species to devour its prey when it is at rest. Two or three times I detected the sound of crunching when a bat was on the wing with a beetle in its mouth, but on these occasions the bat pitched before it dropped the beetle's head and elytra; in one instance only was a beetle wholly devoured without the bat alighting—the discarded portions being dropped in flight; but even in this exceptional case the bat three or four times attempted to find but failed to secure a foothold.

It is very doubtful if the Greater Horseshoe has keen sight; indeed, though the action of the bat when hanging by its feet and fully awake, suggests that it is looking round—a rapid, nervous movement of the head, slightly raised, it does not seem to notice beetles which are held in front of it, or to be able to quickly locate them when they are crawling near it on the ground. I have mentioned that the bats found and ate beetles in their cage. I frequently watched them do this, and also saw them, when at liberty in the room, drop on the floor near a beetle; but even if the beetle was only a couple of inches beyond the bat's muzzle, it did not seem to be able to see it; but if the beetle touched the bat it was secured. The Horseshoe Bats are unable to walk, but they have a habit of dropping with outstretched wings upon a flat surface, from which they spring again with surprising agility. On one occasion a bat dropped near a beetle which had buzzed, for undoubtedly the buzzing of a beetle at once attracted the bat's attention; the bat moved its head to and fro, the lower edge of the horse-

shoe touching the floor. The beetle walked a few inches away, and then again attempted to fly; instantly the bat followed it in a series of little jumps, really short flights of a few inches, and after two or three jumps reached and fell upon the beetle, which it at once thrust into its interbrachial membrane. Directly it had secured the beetle it rose from the floor, flew to a customary perch and, there hanging, consumed it. In the cage the method was similar; the bat dropped on to the floor of the cage, lying with extended wings and either feeling or smelling round—at least that was what the action suggested—until it found a beetle; directly one was secured, it sprang up, turned in the air, and clutched the bar of wood, only twenty inches above it, with its feet. The beetle was then pushed into the wing as usual, and the head and perhaps other fragments dropped. This, then, is evidently the way in which flightless beetles and spiders are caught, and possibly coprophagous beetles may be thus picked up when they are crawling over dung.

This is, however, not the only way in which the Greater Horseshoe secures its food; it can and does catch insects on the wing. *G. typhœus* is a beetle which flies during mild weather in winter, and when I released a dozen beetles in my room in the evening, two or three would quickly attempt to fly. It was when this occurred that I felt certain that the bats hunt and locate their prey mainly by means of their acute hearing. The deep booming buzz of the flying beetle at once roused the Horseshoes to activity, even when, as was often the case after eating two or three beetles, their heads were drooping, and they were relapsing into sleep. Usually the bat left its foothold immediately the beetle began to buzz, and as these beetles are not always quick in getting on to the wing, the bat frequently skimmed over and missed its prey. But

when the beetle had risen two or three inches from the ground, it was doomed ; the bat came down like a falcon stooping, and with marvellous precision caught the flying beetle in its jaws, and carried it off to some place where it could pitch and devour it. For several weeks this performance was repeated on an average two or three times each night, and though on a few occasions the beetle got well into the air before it was captured, by far the greater number were secured before they had risen many inches from the ground.

In order to watch closely the method of devouring food I frequently allowed the bats to hang from my fingers and gave them beetles. If the beetle was small it was eaten without any assistance from the wing ; the larger the beetle the more vigorous the action of the bat. When a large beetle was seized it was at once thrust into the posterior portion of the interbrachial membrane. The claws of the leg on the side thus used were usually released from their hold, and the whole wing brought suddenly forward, by simultaneous stroke of arm and leg, to meet the head. The beetle was practically beaten against the membrane by rapid movement of the bat's head, assisted by the forward stroke of the wing. This wing-action, suggestive of the use of a hand, has no exact parallel in the apparently similar use of the interfemoral pouch by vespertilionid bats. The beetle was moved by the bat against the membrane, for its position in the mouth had frequently to be shifted before the bat could devour the abdomen and reject the head ; and sometimes the action of head and wing together actually pushed the beetle further into the mouth. When the beetle was first seized the wings of the bat were only slightly unfolded, held free but with the membrane partially hiding the body, and when the bat took a beetle from the hand it

beat rapidly with both arms but did not grasp with the thumb. Directly the beetle was in the bat's jaws the wings were further opened, and hung quite loosely whilst the beetle was being devoured.

After a few seconds the head was withdrawn from the wing and the beetle masticated; the rejected portions fell, and the bat, generally suspended by one leg, swung from side to side. This swinging round was even more remarkable when both feet were attached to some hold; the animal could then turn almost completely round, crossing its legs, without altering the position of its feet. When the beetle was finished, the bat usually bent forward, and two or three times touched the object from which it was suspended lightly with its lips; this was especially noticeable when a bat had been feeding when hanging from my hand. Frequently, also, one leg was brought forward, and the teeth scratched or the lips combed by the claws, probably to get rid of some particles of beetle which were sticking to the teeth or lips.

When the beetle was quite finished and the subsequent performances had been gone through, the bat hung, bending its whole body forward, turning from side to side, and moving its head, ears and nose-leaf with great rapidity; it appears to be looking for food, but perhaps searching for prey would be a more correct way of expressing it.

At first the number of beetles eaten per bat was from 5 to 8 each night, but later, when they were regularly feeding themselves, they took so many as 10 or 12, and even occasionally 16 in a night. The number did not depend upon the number left in the cage, for frequently beetles were untouched in the morning; this may, however, have been due to inability on the part of the bats to find and secure them all.

The bats preferred living beetles to dead ones, and though I now and then managed to trick a bat into eating one which had been killed, it was the exception rather than the rule. Generally the bat would over and over again refuse a dead beetle which was offered to it, although it would at once seize one which was alive, even though the beetle was feigning death. It is quite likely that the bat could smell the difference, for a dead *Geotrupes* has a noticeably unpleasant odour.

The Horseshoe drinks by lapping with the tongue. It is a thirsty animal, and we can only suppose, from its behaviour in captivity, that it obtains water in its natural state; possibly, like other bats, it hovers over pools of water and laps whilst on the wing.

The Greater Horseshoe is famous for its wonderful power of flying round and amongst obstacles without touching them with its wings, but when it is dashing after its prey it pays little attention to its surroundings. On one occasion a bat caught a beetle near the floor, and in its headlong dash bumped, with the beetle in its mouth, into the leg of a chair; on another, bat and beetle came full tilt against my waistcoat. Once or twice the bat fell to the ground, but, having secured the beetle in its dash, rose at once with its prey. Occasionally, however, a beetle was captured with more deliberation. The bat would fly to it, hesitate in its flight, hover a second, gently snap the beetle, and at once return to its perch.

The temperature in the Cheddar caves and in the old mines at Braunton is fairly constant, about 52°F. I attempted to keep the temperature of the room in which I kept my bats as even as possible, but found it very difficult to do this during the frosts in January. At Cheddar the bats became active at dusk, but as a rule my captives did not awake until late in the evening, generally

between 11 p.m. and 11-30 p.m. On several nights the bats did not awake at all, and on some only one was active; a table of dates with temperatures in the room and in the open shows a remarkable irregularity, which

Date.	Temp. in Room. Highest.	Temp. in Room. Lowest.	Temp. in Open. 8 a.m. F.	Temp. in Open. 10 p.m. F.	Time of Awaking. No. 1.	Time of Awaking. No. 2.	Number of Beetles Eaten. No. 1.	Number of Beetles Eaten. No. 2.	Remarks.
Dec. 29	...	...	34°	36°	...	...	5	5	Both awoke, but time not taken.
,, 30	...	...	36°	36°	...	...	...	...	Did not wake.
,, 31	...	...	35°	34°	p.m. 10-45	p.m. 11-30	8	5	...
Jan. 1	...	...	36°	35°	...	...	...	...	Did not wake.
,, 2	...	...	30°	32°	...	...	...	...	,, ,,
,, 3	...	...	28°	28°	11-0	11-0	7	6	...
,, 4	...	...	26°	27°	...	11-30	...	8	No. 1 did not wake.
,, 5	...	...	22°	18°	...	...	...	...	Did not wake.
,, 6	50°	38°	39°	45°	11-30	...	5	...	No. 2 did not wake.
,, 7	56°	48°	46°	41°	11-15	11-30	5	5	...
,, 8	54°	46°	39°	39°	...	...	...	...	Did not wake.
,, 9	51°	42°	33°	31°	...	...	...	...	,, ,,
,, 10	48°	44°	25°	25°	...	...	...	...	,, ,,
,, 11	48°	40°	32°	31°	11-0	9-30	6	8	42° at 9-30 p.m. in room.
,, 12	50°	40°	27°	28°	...	9-30	...	2	Only No. 2 woke.
,, 13	50°	40°	28°	38°	...	...	...	...	Did not wake.
,, 14	46°	42°	39°	44°	awoke	awoke	both	fed.	Times not taken.
,, 15	49°	42°	45°	50°	both	awoke,	but	parti	culars not taken.

After this both bats awoke and fed regularly.

can, I fancy, only be explained by the altered conditions under which the bats were living in confinement. On the night of the 8th, for instance, when the temperature

had been high during the previous 24 hours, neither bat awoke, but on the night of the 11th, when the temperature had been, for the room, decidedly low, both bats awoke and fed. When the average temperature had been practically the same on the three nights of 11th, 12th, and 13th, both awoke on one night, one on the next, and neither on the third.

Sight, in the Greater Horseshoe, is, as I have said, apparently not acute. When we compare the small eyes, almost buried behind the facial ornaments, with the large prominent eyes of the Long-eared Bat, *Plecotus auritus*, Geoffr., a species which apparently uses its eyes when feeding, we can hardly imagine that they are of much service to the animal. Nevertheless it not only locates flying food with remarkable certainty (I have suggested by sound), but can at once discover the exact situation occupied by another bat. Frequently when both bats were at liberty in the room, and one had pitched and was quiet, the other would fly to it without hesitation and pitch beside it or actually upon it. On such occasions there was nothing to suggest that the one bat heard the other, but when one was crunching a beetle, and the other hovered round it, pitched near it, and, as happened more than once, took a portion of the uneaten beetle from the mouth of the original captor, it may easily have been guided by sound.

The normal position of the sleeping Greater Horseshoe is similar to that of the Lesser Horseshoe (4), which has been frequently described, but the tail is often more visible, standing out at an angle of about 30°. When the bat is disturbed in sleep it draws itself up by bending the legs (as shown in the photograph taken in a Cheddar cave), and when slowly awakening will hang with the legs bent and the wings slightly unfolded. The bat pants and

throbs, especially in the abdominal region, but it is not until it is nearly awake that it begins to move its head, ears, and facial adornment. When fully awake these are in constant rapid movement.

The conclusions in my former paper (1) were confirmed by the behaviour of my recent captives. I remarked (p. 322):—"1. The Greater Horseshoe, if the weather be open at the end of December and beginning of January, is not in a state of hibernation. It moves in the caves, awakening without artificial stimulus, and leaves the caves apparently in search of food."

In captivity we cannot get rid of "artificial stimulus" entirely, but my bats awoke, as a rule naturally, night after night, but during the frosts they occasionally slept for one, two, or three nights without awakening, although the temperature in the room was considerably higher than in the open.

"4. Food is conveyed into the caves from without and devoured there, the bats hanging whilst they feed."

My bats certainly show that it is a rule to carry food to some foothold before devouring it.

"5. Certain creatures are captured and eaten in the caves."

"6. Creatures incapable of flight are captured by the bats and devoured."

By seeing the bats pick up beetles from the carpet or from the floor of the cage I was quite satisfied that they find and catch spiders and flightless beetles in this way, in the caves and probably also in the open. But from the way in which the bats skilfully secured flying beetles it is perfectly evident that in the free state they constantly secure their food on the wing. Mr. Bruce F. Cummings kept, for a few days, a Lesser Horseshoe Bat

which caught house-flies in a room in exactly the same way.

"7. When feeding the Greater Horseshoe makes use of the interbrachial membrane and not of the interfemoral pouch."

This was confirmed by a much larger number of experiments than I was able to make in 1907.

REFERENCES.

1. *Proc. Zool. Soc.*. 1907, pp. 312—324.

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4. Oldham, *Manchester Memoirs*, vol. 49, No. 9, pp. 6, 7 (1905).

EXPLANATION OF PLATES.

1. Ventral view of Greater Horseshoe Bat in normal sleeping position.

2. Greater Horseshoe photographed in cave at Cheddar. This bat had not been touched, but it had drawn itself up by bending its legs.

3. Bat awakening from sleep; the body was moving spasmodically, and the ears are slightly bent.

4. Bat devouring a beetle; hanging by one leg; the interbrachial membrane on the left side was in use in this case, and the left foot is hanging free; the head was in rapid motion.

throbs, especially in the abdominal region, but it is not until it is nearly awake that it begins to move its head, ears, and facial adornment. When fully awake these are in constant rapid movement.

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XII. Action of Selenium and Tellurium on Arsine and Stibine.

By Francis Jones, M.Sc., F.R.S.E.

Received and Read March 10th, 1908.

In a paper on Stibine, read before the Chemical Society of London and published in the *Journal* for 1876, page 641, I pointed out that sulphur decomposes the gas in presence of light, with formation of hydrogen sulphide and antimony trisulphide which deposits on the sulphur. Further that the liberated hydrogen sulphide also reacts with stibine producing antimony trisulphide and free hydrogen.

$$(a) \quad 2SbH_3 + 6S = Sb_2S_3 + 3H_2S.$$

$$(b) \quad 2SbH_3 + 3H_2S = Sb_2S_3 + 12H.$$

It was shown that the reaction serves as an extremely delicate test for stibine, and it was also used to determine its composition, by estimating the amount of antimony deposited as sulphide on the sulphur, as compared with the amount of hydrogen in the hydrogen sulphide evolved. The value of this method for determining the compcsition of certain hydrides, has since been shown by its application to the case of germanium hydride which is also decomposed by sulphur. [*Zeitschrift für Anorganische Chemie*, vol. 30, page 325, Über Germaniumwasserstoff von E. Voegelen.]

I also showed that sulphur acts similarly on phosphine and arsine, but not so readily as in the case of stibine.

April 22nd, 1908.

when the beetle had risen two or three inches from the ground, it was doomed ; the bat came down like a falcon stooping, and with marvellous precision caught the flying beetle in its jaws, and carried it off to some place where it could pitch and devour it. For several weeks this performance was repeated on an average two or three times each night, and though on a few occasions the beetle got well into the air before it was captured, by far the greater number were secured before they had risen many inches from the ground.

In order to watch closely the method of devouring food I frequently allowed the bats to hang from my fingers and gave them beetles. If the beetle was small it was eaten without any assistance from the wing ; the larger the beetle the more vigorous the action of the bat. When a large beetle was seized it was at once thrust into the posterior portion of the interbrachial membrane. The claws of the leg on the side thus used were usually released from their hold, and the whole wing brought suddenly forward, by simultaneous stroke of arm and leg, to meet the head. The beetle was practically beaten against the membrane by rapid movement of the bat's head, assisted by the forward stroke of the wing. This wing-action, suggestive of the use of a hand, has no exact parallel in the apparently similar use of the interfemoral pouch by vespertilionid bats. The beetle was moved by the bat against the membrane, for its position in the mouth had frequently to be shifted before the bat could devour the abdomen and reject the head ; and sometimes the action of head and wing together actually pushed the beetle further into the mouth. When the beetle was first seized the wings of the bat were only slightly unfolded, held free but with the membrane partially hiding the body, and when the bat took a beetle from the hand it

beat rapidly with both arms but did not grasp with the thumb. Directly the beetle was in the bat's jaws the wings were further opened, and hung quite loosely whilst the beetle was being devoured.

After a few seconds the head was withdrawn from the wing and the beetle masticated; the rejected portions fell, and the bat, generally suspended by one leg, swung from side to side. This swinging round was even more remarkable when both feet were attached to some hold; the animal could then turn almost completely round, crossing its legs, without altering the position of its feet. When the beetle was finished, the bat usually bent forward, and two or three times touched the object from which it was suspended lightly with its lips; this was especially noticeable when a bat had been feeding when hanging from my hand. Frequently, also, one leg was brought forward, and the teeth scratched or the lips combed by the claws, probably to get rid of some particles of beetle which were sticking to the teeth or lips.

When the beetle was quite finished and the subsequent performances had been gone through, the bat hung, bending its whole body forward, turning from side to side, and moving its head, ears and nose-leaf with great rapidity; it appears to be looking for food, but perhaps searching for prey would be a more correct way of expressing it.

At first the number of beetles eaten per bat was from 5 to 8 each night, but later, when they were regularly feeding themselves, they took so many as 10 or 12, and even occasionally 16 in a night. The number did not depend upon the number left in the cage, for frequently beetles were untouched in the morning; this may, however, have been due to inability on the part of the bats to find and secure them all.

It appeared probable that the elements selenium and tellurium, which are analogous to sulphur, would act in a similar way with arsine and stibine and I have now to record the results of experiments made to decide this question.

In the course of these experiments it was necessary to have distinguishing tests for the hydrides of sulphur, selenium and tellurium which are so alike in their reactions. Each of these hydrides produces on paper moistened with solution of lead acetate, dark stains which are extremely similar, and I was unable to find any tests recorded which would serve to distinguish them. On trying the effect of various reagents upon these stains I found that hydrogen dioxide and hydrochloric acid were useful for this purpose, at all events in the case of a distinct stain. The differences in the behaviour of these stains with reagents are shown in the following table :—

Stain.	H_2O_2.	HCl. (dilute).	HCl. (strong).
Lead Sulphide.	Stain rapidly fades and leaves paper white.	Stain fades almost instantly and leaves paper white.	Stain instantly fades and leaves paper white.
Lead Selenide.	Stain slowly fades and leaves orange coloured stain (selenium)	Stain slowly fades and leaves orange coloured stain.	Stain slowly fades and leaves orange coloured stain.
Lead Telluride.	Stain fades in course of time.	Stain fades only slightly and leaves greyish black stain.	Stain fades only slightly and leaves greyish black stain.

These reactions were employed to discriminate between the lead stains obtained in the course of the

experiments, except in those in which arsine was used, as this gas acts directly on solutions of lead and produces stains on paper moistened with lead acetate very similar to the others. This reaction will be referred to subsequently.

Action of Selenium on Arsine.

(*a*) *In sunlight.*

The arsine employed was generated in the way described in the paper already referred to, that is to say, a solution of arsenious acid in dilute hydrochloric acid was allowed to drop from a stoppered funnel tube into a gas bottle filled with pure granulated zinc. The exit tube from the gas bottle passed to a wash bottle containing dilute soda, and the exit tube from this passed to a drying tube filled with calcium chloride. The gas so obtained then passed into a tube containing powdered selenium exposed to sunlight. Under these circumstances the selenium became slowly coated with a deposit of arsenic selenide, arsenic being easily detected in the contents of the tube.

A reaction exactly similar to that occurring when arsine is passed over sulphur exposed to light therefore takes place, although more slowly:

$$2AsH_3 + 6Se = As_2Se_3 + 3H_2Se.$$

Just as in the sulphur experiments, a secondary reaction was found to take place between the liberated hydrogen sulphide and the arsine, resulting in the formation of arsenic sulphide (which deposited on the glass tube) and free hydrogen, so in the selenium experiment, after prolonged exposure to sunlight, the glass tube was found slightly coated with arsenic selenide, due to the reaction of the liberated hydrogen selenide on the arsine, thus

$$2AsH_3 + 3H_2Se = As_2Se_3 + 12H.$$

(*b*) *At a temperature of 100°C.*

Arsine was passed over selenium contained in a U-tube which was immersed in boiling water, and the whole apparatus was screened from daylight. There was no difference in the appearance of the tube at the conclusion of the experiment, and on testing the contents the absence of arsenic was proved, so that arsine is not decomposed by selenium at the temperature of boiling water.

(*c*) *In the dark.*

Arsine was passed over selenium contained in a tube screened from light. The gas was passed for several hours but no reaction occurred.

Action of Tellurium on Arsine.

(*a*) *In sunlight.*

Arsine was passed for three hours over powdered tellurium contained in a straight glass tube. At the end of the experiment there was no visible change in the appearance of the tellurium, but on removing it from the tube and heating it in a test tube, a sublimate formed which, under the microscope, was seen to consist of distinct octahedral crystals. These were dissolved in water, hydrochloric acid and sulphuretted hydrogen were added, and a yellow precipitate of arsenic tri-sulphide appeared. So that in presence of sunlight, tellurium decomposes arsine.

(*b*) *At a temperature of 100°C.*

Arsine was passed over powdered tellurium contained in a U-tube protected from light and kept at a temperature of 100°C. After more than three hours treatment the action was stopped and the contents of the tube tested

for arsenic as in the preceding experiment, but not a trace of arsenic was detected.

(*c*) *In the dark.*

The experiment was repeated at the ordinary temperature for the same length of time, but no arsenic could be detected in the contents of the tube.

Action of Selenium on Stibine.

(*a*) *In sunlight.*

Stibine, prepared in a similar manner to arsine as above, was passed over powdered selenium contained in a glass tube exposed to sunlight. The escaping gas was passed over paper moistened with solution of lead acetate. This very soon blackened and the stains produced were not rapidly removed either by hydrogen dioxide or hydrochloric acid, and after a time they became orange coloured owing to separation of selenium. During the experiment it was noticed that a brown film slowly formed on the glass tube containing the selenium, showing that in this case also a secondary reaction occurred between the liberated hydrogen selenide and the stibine:

$$2SbH_3 + 3H_2Se = Sb_2Se_3 + 12H.$$

(*b*) *At a temperature of 100° C.*

Stibine was then passed over powdered selenium contained in a U-tube immersed in boiling water and screened from light.

As the selenium melts so readily that it might stop the passage of the gas, small portions were placed in the U-tube, separated by plugs of cotton wool. At the exit a tube was placed containing paper moistened with solution of lead acetate. Soon after the experiment began, the lead paper became stained and eventually was

blackened all over. The stains were not removed by treatment with either hydrogen dioxide or hydrochloric acid, so that selenium decomposes stibine at the temperature of boiling water.

(*c*) *In the dark.*

Stibine was passed over powdered selenium contained in a straight glass tube and screened from light. A paper moistened with lead acetate solution was placed between the stibine apparatus and the selenium tube, and another at the exit of the apparatus. When the gas had passed for half an hour the first paper was quite unaltered, but the second was stained half its length. The experiment was continued and ultimately the second paper was blackened all over. The stains were not removed by hydrochloric acid, so that, even in the dark, stibine is decomposed by selenium. This reaction was so remarkable and unexpected that the experiment was several times repeated with the same result. Of all the hydrides examined in contact with sulphur, selenium, and tellurium, stibine is the only one to be decomposed in the dark at the ordinary temperature, and then only when in contact with selenium.

Action of Tellurium on Stibine.

(*a*) *In sunlight.*

Stibine was passed over powdered tellurium contained in a straight glass tube and exposed to sunlight. The exit tube contained a paper moistened with solution of lead acetate. This paper became darkened after the prolonged action of the gas, and the stains were not removed when placed in hydrochloric acid, so that stibine is slowly decomposed in presence of tellurium exposed to sunlight.

(*b*) *At a temperature of 100°C.*

When stibine was passed over tellurium kept at a temperature of 100°C., the lead paper was distinctly stained after the prolonged passage of the gas, and the stains were not removed by hydrochloric acid.

(*c*) *In the dark.*

In a similar manner, stibine was passed over tellurium, but in the dark and at the ordinary temperature no reaction took place.

The results of the investigation, including those of the previous paper in which sulphur was used, may be tabulated as follows:—

In sunlight.

Element.	Hydride.	Result.	Element.	Hydride.	Result.
Sulphur	AsH_3	Reacts slowly	Sulphur.....	SbH_3	Reacts easily
Selenium.....	AsH_3	Reacts	Selenium ...	SbH_3	Reacts easily
Tellurium ...	AsH_3	Reacts	Tellurium ...	SbH_3	Reacts
		At 100°C.			
Sulphur	AsH_3	Reacts slowly	Sulphur.....	SbH_3	Reacts slowly
Selenium.....	AsH_3	No action	Selenium ...	SbH_3	Reacts slowly
Tellurium ...	AsH_3	No action	Tellurium ...	SbH_3	Reacts very slowly
		In the dark.			
Sulphur	AsH_3	No action	Sulphur.....	SbH_3	No action
Selenium.....	AsH_3	No action	Selenium ...	SbH_3	Reacts
Tellurium ...	AsH_3	No action	Tellurium ...	SbH_3	No action

In the previous paper I pointed out that the facility of the decomposition of the hydrides of phosphorus, arsenic, and antimony by means of sulphur when exposed to light, increased with the rise in the molecular weight of these gases, phosphine being very slowly decomposed, arsine more rapidly, and stibine very rapidly. Similarly, it is now

to be noted that the action of these hydrides on sulphur, selenium, and tellurium corresponds with the rise in the atomic weights of these elements. This is shown by the comparative rapidity with which the action takes place in each case, stibine being readily decomposed by selenium and more slowly by tellurium exposed to sunlight, whereas arsine reacts on selenium slowly and on tellurium only very slowly. The fact that stibine is decomposed by selenium, even in the dark is, of course, a marked exception. I have previously shown that paper coated with sulphur when in contact with stibine and exposed to light, may be used like sensitive paper in photographic printing, the exposed portions becoming orange coloured owing to the deposition of antimony trisulphide, the protected parts retaining the yellow colour of the sulphur. In a similar way paper coated with selenium and kept in contact with stibine may be used as printing paper, but there is, of course, not so great a contrast between the colour of the antimony selenide deposited and the selenium, as there is between the orange-coloured antimony trisulphide and the sulphur.

Action of Arsine on solutions of Lead.

I have already referred to the action of arsine on solution of lead acetate as preventing the adoption of the lead paper test with this gas. It is surprising that this reaction seems hitherto to have escaped observation, seeing how thoroughly the behaviour of both arsine and stibine with reagents has been investigated. In Gmelin's "Handbook," vol. 4, pages 268 and 335, it is stated on the authority of Simon that arsine and stibine do not act on lead acetate, and I have not found any other statement on the subject. In the course of the experiments above described I found in one case such a

rapid blackening of the lead paper, that it occurred to me that it might be due to the arsine itself and not to any reaction on the selenium that was being exposed to it. This led me to remove the selenium tube and allow the gas to act on a fresh paper moistened with solution of lead acetate. This was rapidly darkened and the stain had very much the appearance of papers stained with lead selenide and telluride. These stains are distinguished from those due to lead selenide and telluride by not altering when treated with either strong or dilute hydrochloric acid.

On allowing arsine to bubble through solution of lead acetate contained in a wash bottle, the liquid soon darkens in colour and a black precipitate separates. Subsequently a black shining mirror forms on the sides of the bottle and adheres firmly to the glass for some days, when it drops off.

I can find nothing in the precipitate but lead and arsenic, but on making quantitative determinations I found such varying proportions of the two elements in different preparations, that I could only conclude that the compound is probably an arsenide of lead but not of definite composition.

Similar experiments with stibine showed that this gas has no action on solution of lead acetate.

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XIII. Report on the Recent Foraminifera from the Coast of the Island of Delos (Grecian Archipelago). Part V.

By HENRY SIDEBOTTOM.

(Received and read March 10th, 1908.)

Uvigerina, d'Orbigny.

Uvigerina canariensis, d'Orbigny (Pl. 1, figs. 1, 2).

Uvigerina canariensis, d'Orbigny ('39), p. 138, pl. 1, figs. 25-27.

U. urnula, d'Orbigny ('46), p. 189, pl. 11, figs. 21, 22.

U. irregularis, Brady ('65), p. 100, pl. 12, fig. 5.

There is a certain amount of suspicion attached to the identification of these specimens. Very rare.

Uvigerina tenuistriata, Reuss (Pl. 1, fig. 3).

Uvigerina tenuistriata, Reuss ('70), p, 485, — Schlicht ('70), pl. 22, figs. 34, 37.

U. tenuistriata (Reuss), Brady ('84), p. 574, pl. 74, figs. 4-7.

Although not quite typical, the contour of the test is sufficient to bring it under this heading. There are very faint indications on some of the chambers of the striæ which are present in the type. Only one was found.

***Uvigerina angulosa,** Williamson (Pl. 1, fig. 4).

Uvigerina angulosa, Williamson ('58), p. 67, pl. 5, fig. 140.

* The asterisk denotes that this species occurs at Palermo.

May 23rd, 1908.

cause the test to be rugose. Along the sutural lines the pores hardly show, and so the tests have a more or less striped appearance. A good deal of exogenous shell growth is present in the umbilical region of the larger specimens, the inferior surface being decorated with radiating lines of minute tubercles. Rather frequent.

Mr. Millett figures a variety of *D. imperatoria* in his Malay report (:**03**), pl. 7, fig. 6, under the name of *Discorbina imperatoria*, d'Orb., var. *globosa.*

Discorbina patelliformis, Brady. (Pl. 5, fig. 3.)

Discorbina patelliformis, Brady ('**84**), p. 647, pl. 88, fig. 3 and pl. 89, fig. 1.

D. patelliformis (Brady), Egger ('**93**), p. 390, pl. 15, figs. 48-50.

Two of the specimens are particularly elegant; they are the largest of those found, and have the chambers of the last two whorls inflated, the peripheral outline being much lobulated, and the pores more marked. Very rare.

Discorbina pulvinata, Brady. (Pl. 5, fig. 4).

Discorbina pulvinata, Brady ('**84**), p. 650, pl. 88, fig. 10.

D. pulvinata (Brady), Egger ('**93**), p. 391, pl. 15, figs. 33-35.

Brady, in his provisional description of this species, puts the number of chambers in the last convolution at about three. The Delos specimens have five, with the exception of two or three, which have six. I have specimens also from off the island of Rhodes. Very frequent.

A variety occurs which has only three or four chambers in the last convolution, these latter are not nearly so compressed, and the chambers on the superior surface are difficult to distinguish owing to exuberant shell growth. In colour they are white. Very rare.

Discorbina pileolus, d'Orbigny, sp.

Valvulina pileolus, d'Orbigny ('**39**), p. 47, pl. 1, figs. 15-17.

Discorbina pileolus (d'Orb.), Brady ('**84**), p. 649, pl. 89, figs. 2-4.

Unfortunately the six tests found are in the condition known as plastogamy, thus forming three pair. I think there can be little doubt that they belong to this species, though not quite typical. In two of them, however, a small portion of the inferior surfaces can be seen (the tests not being of quite the same size), and they appear to bear the characteristic markings.

***Discorbina tabernacularis**, Brady.

Discorbina tabernacularis, Brady ('**84**), p. 648, pl. 89, figs. 5-7.

D. tabernacularis (Brady), Egger ('**93**), p. 390, pl. 15, figs. 58-60, 79.

The Delos specimens agree best with the smaller of those figured by Brady in the above reference, viz., fig. 7, with the exception that the delicate striæ are absent.

Mr. Millett (:**03**) reports it from the Malay Archipelago, page 700. Very rare.

***Discorbina tuberculata**, Balkwill and Wright.
(Pl. 5, fig. 5).

Discorbina tuberculata, Balkwill and Wright ('**85**), p. 350, pl. 13, figs. 28-30.

D. tuberculata (B. & W.), Halkyard ('**89**), p. 70, pl. 2, fig. 10.

The tubercles are not so large as those on the specimen figured by Messrs. Balkwill and Wright, otherwise the specimens are typical. Rather rare.

Discorbina erecta, n. sp. (Pl. 5, figs. 6, 7).

Test free; the contour is that of a tall cone, armed with a short spine at the apex. The segments are inflated, and very rugose, and arranged in about six convolutions, the final whorl consisting of about six segments. The sutural lines are wide and sunk. The inferior surface is more or less rounded, and ornamented by radiating riblets or granulose lines. The umbilicus is deeply sunk.

The tests vary in the height of the spire and amount of rugosity. In some few of the smaller, and presumably younger tests, some of the segments are armed with a small spine. Most probably the extreme rugosity of the larger tests is the result of age. The majority of the specimens are of a greyish-white colour, but a few are tinged with brown. Frequent. This species occurs also off the island of Rhodes.

Discorbina elegantissima, n. sp. (Pl. 5, fig. 8.)

The test is composed (in the specimen figured) of fully three convolutions, the final whorl consisting of four segments, which are more outspread than the others. The segments are inflated, the earlier ones short, and the later ones long and arched. The test is opaque (except the last one or two chambers, which are semi-opaque), and the colour a light yellow-brown; it is also rugose.

Six examples of this interesting and handsome foraminifer were found. The one figured is much the largest of the set. The superior surface has a sugary look, and the chambers are lobulated, and the sutures deeply sunk. The inferior surface is slightly convex, and ornamented with radiating lines of minute tubercles. The umbilical region is sunk and obscured by the shell growth.

In the example figured, the last convolution is more outspread than in the other ones.

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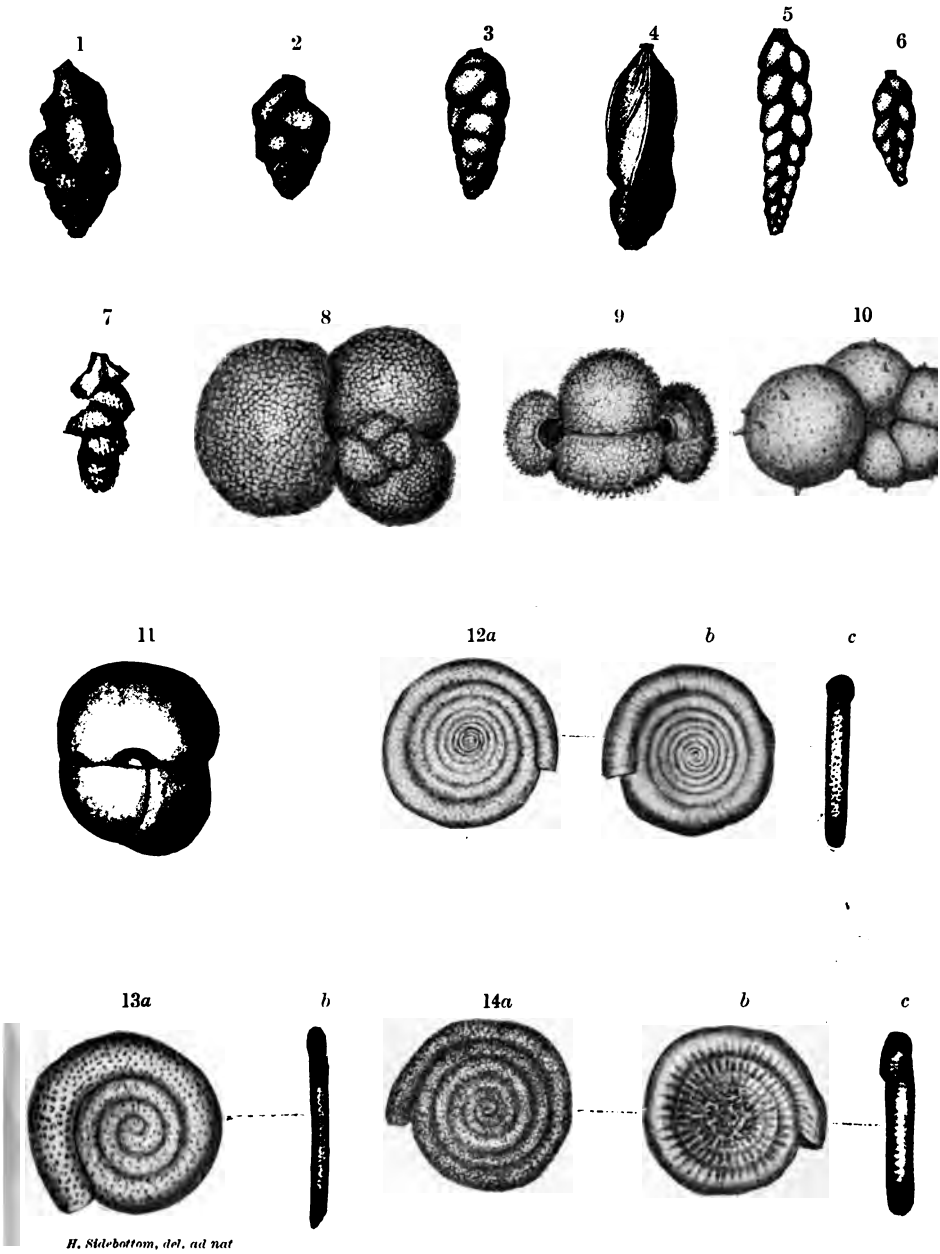
EXPLANATION OF PLATES.

PLATE I.

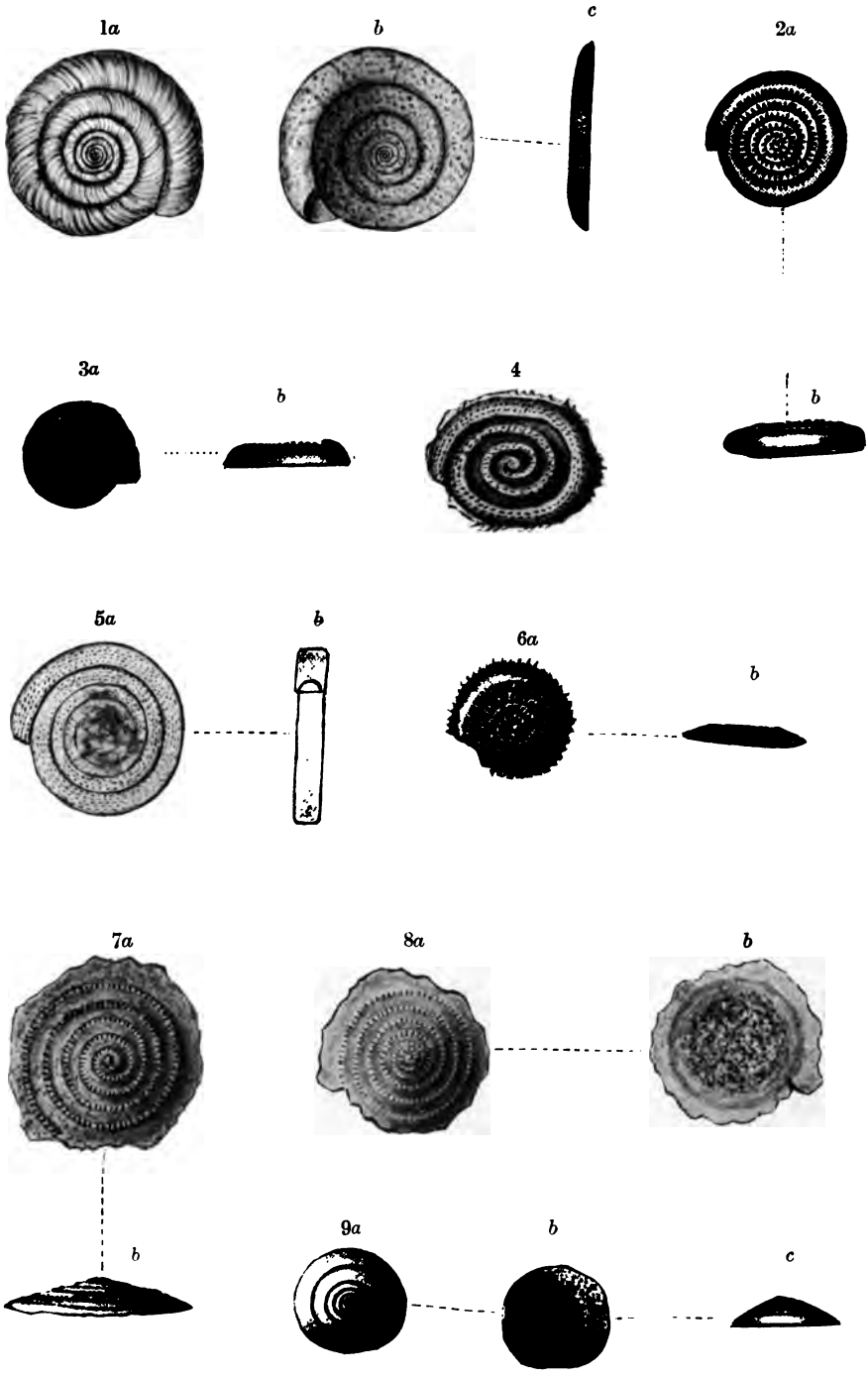
FIGS.			PAGE.
1, 2.	*Uvigerina canariensis*, d'Orbigny	× 75 ...	1
3.	„ *tenuistriata*, Reuss	× 25 ...	1
4.	„ *angulosa*, Williamson	× 75 ...	1
5, 6.	„ *auberiana*, d'Orbigny, var. *glabra*, Millett	× 75 ...	2
7.	*Uvigerina*, sp.	× 75 ...	2
8.	*Globigerina triloba*, Reuss	× 50 ...	3
9.	„ *helicina*, d'Orbigny	× 50 ...	4
10.	„ *æquilateralis*, Brady	× 50 ...	4
11.	*Sphæroidina bulloides*, d'Orbigny	× 50 ...	5
12-14.	*Spirillina vivipara*, Ehrenberg and varieties	× 75 ...	6



Foraminifera from the coast of the island of Delos.

PLATE II.

FIGS.				PAGE.
1—3.	*Spirillina*	*vivipara*, Ehrenberg, varieties	× 75 ...	6
4.	,,	*vivipara*, Ehrenberg, var. *carinata*, Halkyard	× 75 ...	8
5.	,,	*vivipara*, Ehrenberg, var. *complanata*, Jones and others, var.	× 50 ...	8
6.	,,	*decorata*, Brady, var.	× 75 ...	8
7, 8.	,,	*ornata*, n. sp.	× 75 ...	9
9.	,,	*lucida*, n. sp.	× 75 ...	9

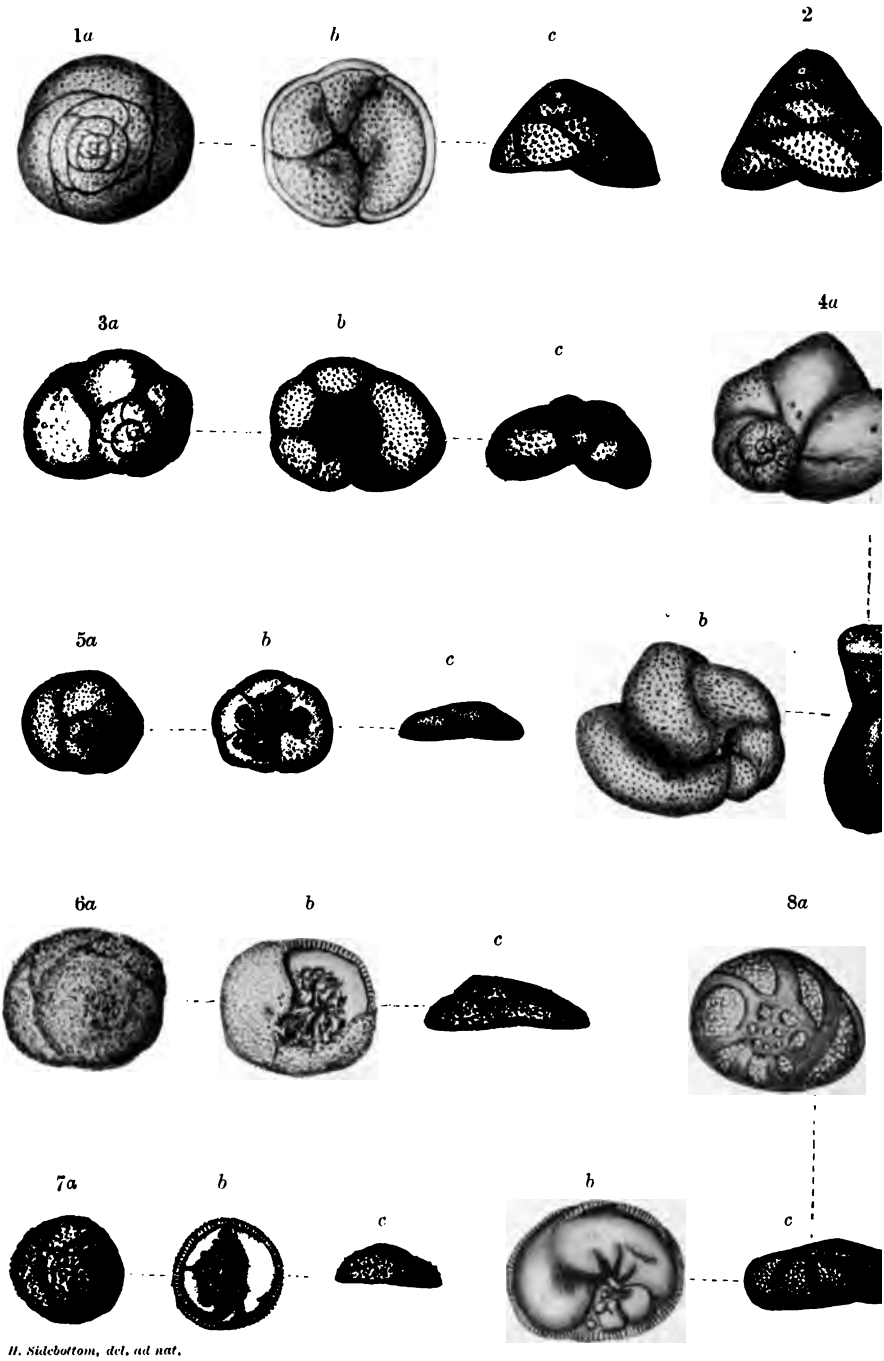


H. Sidebottom, del. ad nat.

Foraminifera from the coast of the island of Delos.

PLATE III.

FIGS.			PAGE.
1, 2.	*Discorbina turbo*, d'Orbigny, sp.	× 50 ...	11
3, 4.	„ *globularis*, d'Orbigny, sp.	× 50 ...	11
5-7.	„ „ „	× 25 ...	11
8.	„ „ „	× 50 ...	11

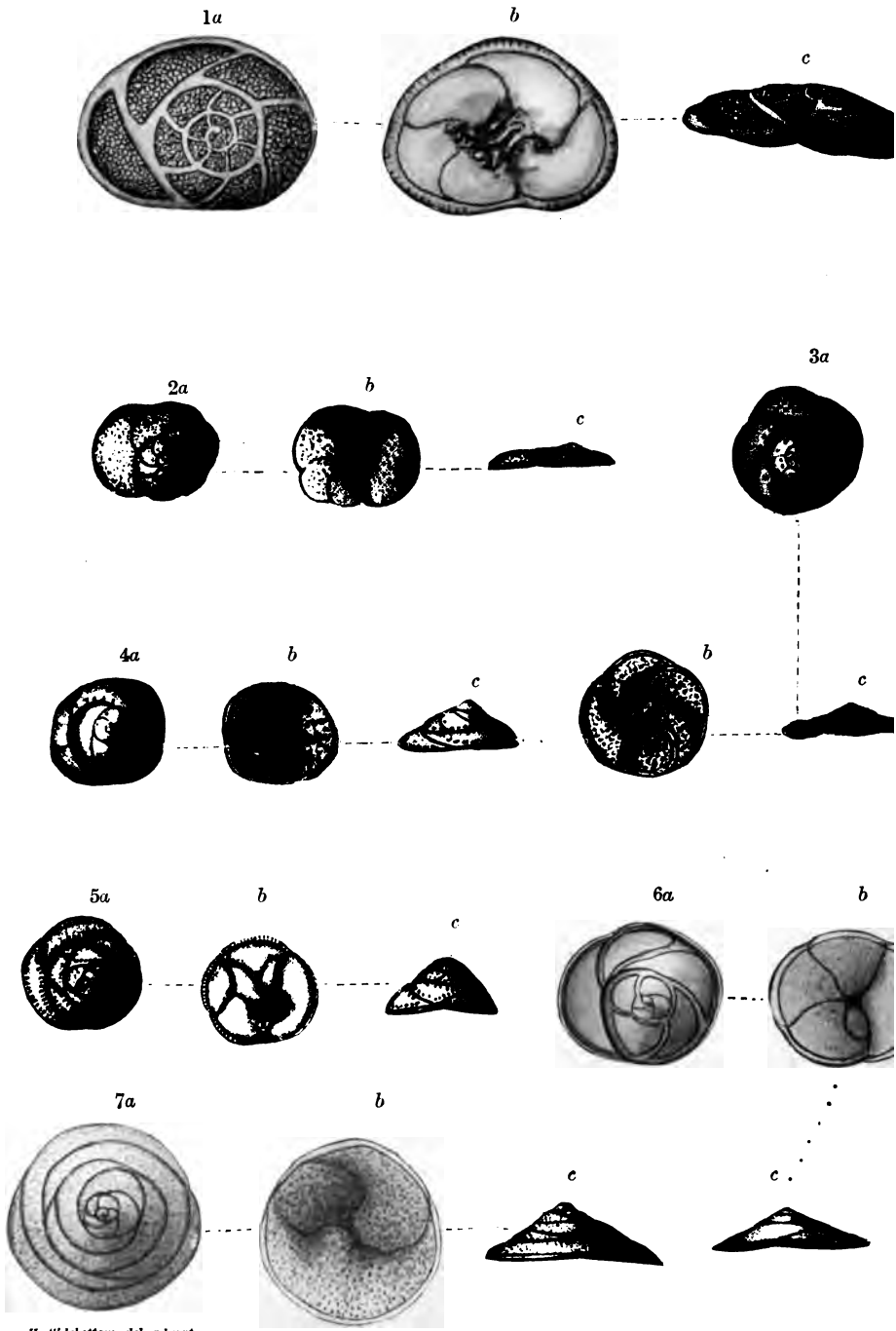


H. Sidebottom, del. ad nat.

Foraminifera from the coast of the island of Delos.

PLATE IV.

FIGS.			PAGE.
1, 2.	*Discorbina globularis*, d'Orbigny, sp.	× 50	11
3, 4.	„ *rosacea*, d'Orbigny sp.	× 25	12
5.	„ „ „	× 50	12
6.	„ *nitida*, Williamson	× 50	13
7.	„ *orbicularis*, Terquem sp.	× 50	13

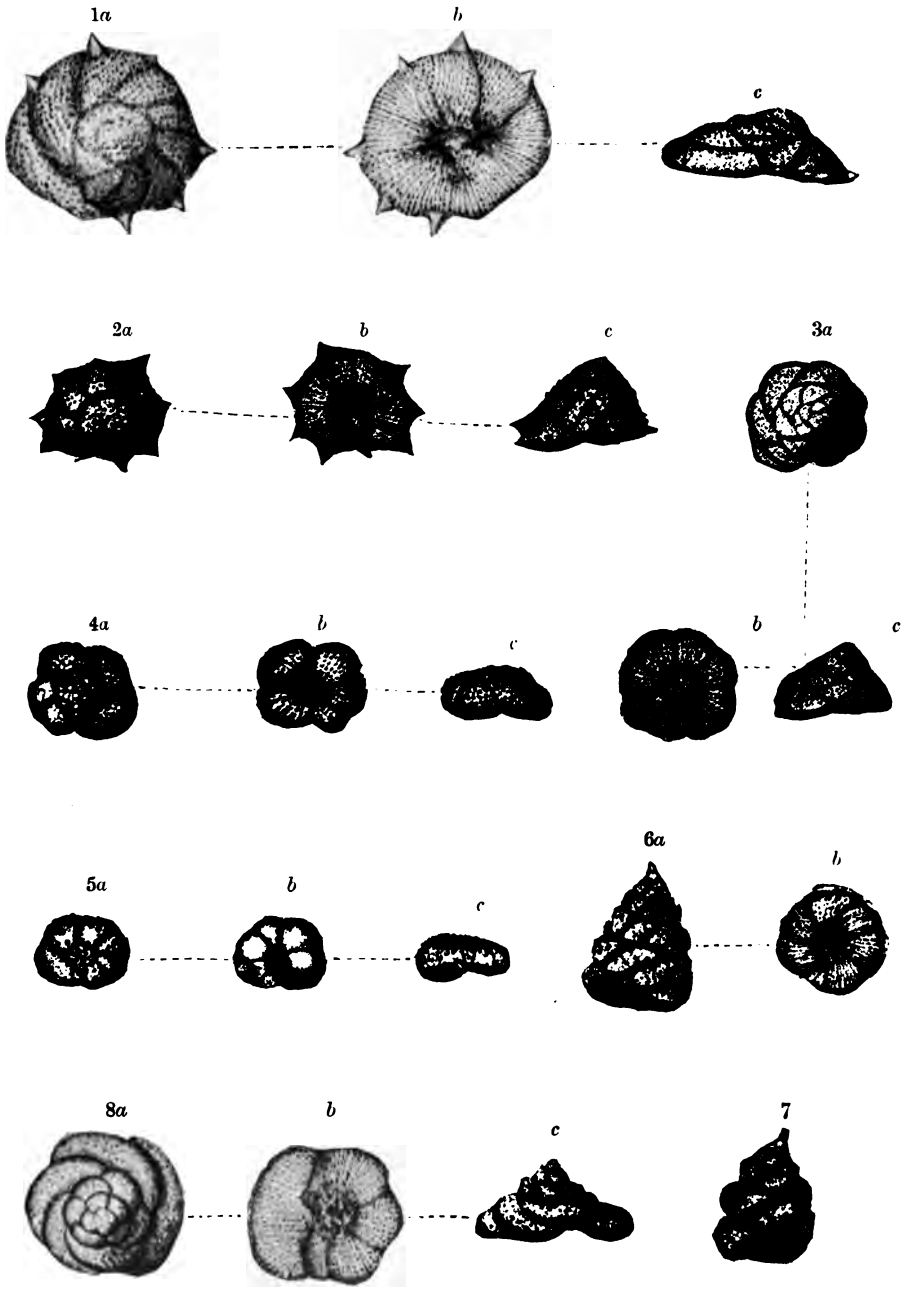


H. Sidebottom, del. ad nat.

Foraminifera from the coast of the island of Delos.

PLATE V.

FIGS.			PAGE.
1, 2.	*Discorbina imperatoria*, d'Orbigny	× 75	13
3.	„ *patelliformis*, Brady	× 75	14
4.	„ *pulvinata*, Brady	× 75	14
5.	„ *tuberculata*, Balkwill & Wright	× 75	15
6, 7.	„ *erecta*, n. sp.	× 50	16
8.	„ *elegantissima*, n. sp.	× 50	16

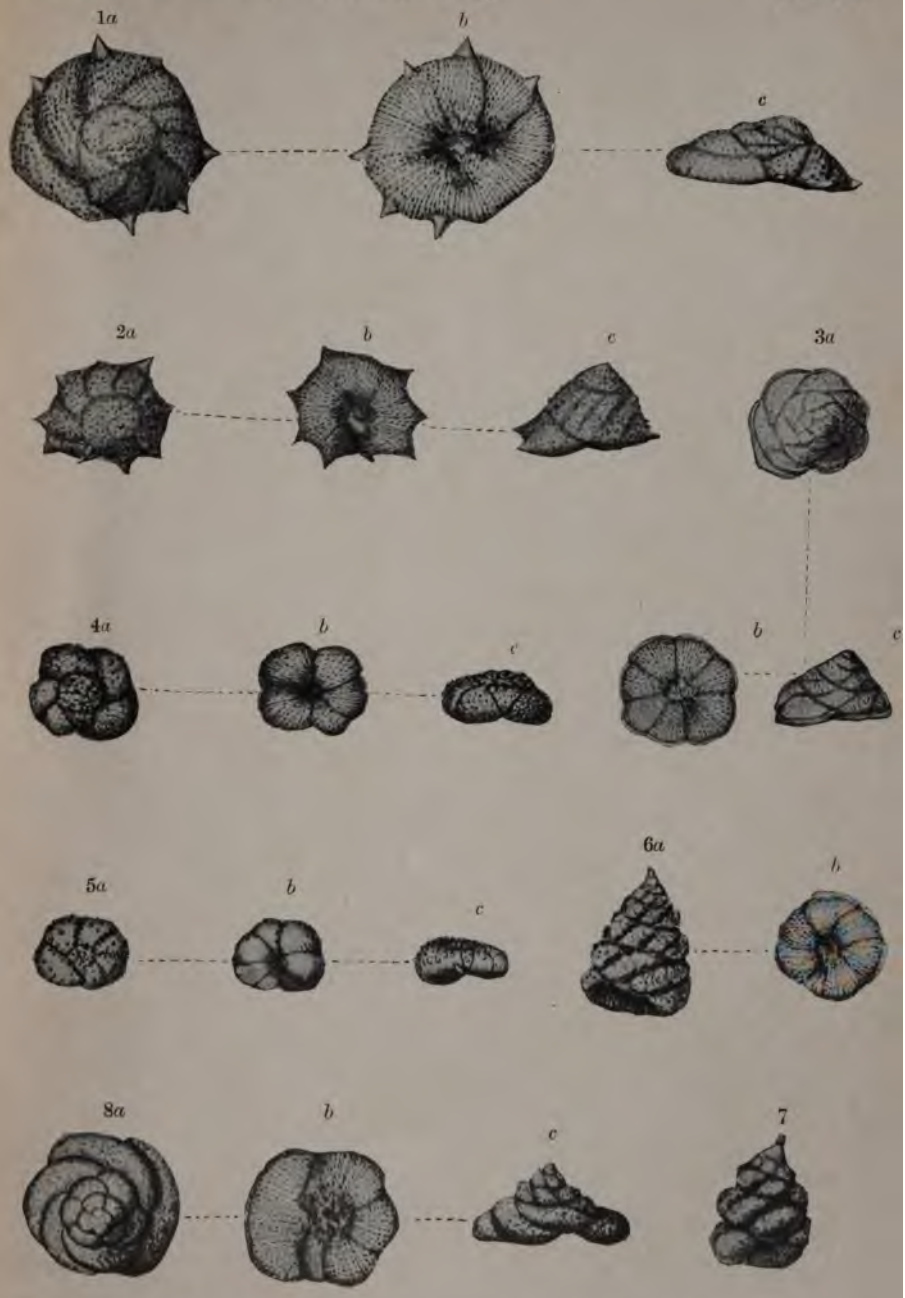


H. Sidebottom, del. ad nat.

Foraminifera from the coast of the island of Delos.

PLATE V.

FIGS.			PAGE.
1, 2.	*Discorbina imperatoria*, d'Orbigny	× 75	13
3.	„ *patelliformis*, Brady	× 75	14
4.	„ *pulvinata*, Brady	× 75	14
5.	„ *tuberculata*, Balkwill & Wright	× 75	15
6, 7.	„ *erecta*, n. sp.	× 50	16
8.	„ *elegantissima*, n. sp.	× 50	16



H. Middlebottom, del. ad nat.

Foraminifera from the coast of the island of Delos.

Manchester Memoirs, Vol. lii. (1908), *No.* **14.**

XIV. Some Observations on the Chemical Action of Tropical Sunlight.

By GILBERT JOHN FOWLER, D.Sc., F.I.C.

Received and Read April 28th, 1908.

In the early part of 1906 the author was called upon to undertake certain investigations in connection with the treatment of sewage, on behalf of the Government of Bengal. The work was carried out for the most part in Calcutta, and in the course of it, it became of importance to determine the effect of tropical sunlight upon the accuracy of iodine titrations, and upon the stability of solutions of chloride of lime.

Some quantitative record of the photo-chemical intensity of the light was thus desirable. The most usual method of photo-chemical measurement is by means of standard silver chloride paper as originally suggested by Bunsen and Roscoe.* Special paper is, however, required to obtain satisfactory results by this method.

Of the various forms of chemical actinometer, that generally referred to as Eder's is fairly well known. Eder determines under defined conditions the rate of reduction of mercuric chloride to mercurous chloride by the action of light in presence of ammonium oxalate. The suggestion that this reaction could be used as a means of measuring the photo-chemical intensity of light was first made by the present writer's father, R. J. Fowler, in a paper read before the British Association in Leeds in 1858.†

* *Pogg. Ann.*, vol. 124, 1865, p. 353.

† *Report Brit. Assoc.*, Leeds, 1858, Chemistry, p. 47.

June 4th, 1908.

This method is, however, not so easily or rapidly carried out, and consequently not so well suited for determinations away from a laboratory, as that used by Dr. Bailey and his colleagues several years ago in Manchester. The occasion, indeed, at once suggested itself as an interesting opportunity for comparing the chemical intensity of sunlight in Calcutta and elsewhere with the records obtained by Dr. Bailey.*

The method used by Dr. Bailey is founded upon a reaction originally described by Dr. Albert R. Leeds, of Philadelphia, and independently by Dr. Angus Smith,† viz., the liberation of iodine from an acid solution of potassium iodide in presence of air and light according to the following equation :—

$$2HI + O = H_2O + I_2.$$

Dr. Angus Smith published a number of preliminary observations as to the effect of temperature, strength of solution, &c., upon this reaction.† He also gave results of the measurement of actinic sunshine in Manchester for a year, and a comparison of the transparency of the Manchester atmosphere in town, suburb, and country.‡

The method, as described by Dr. Angus Smith, was modified slightly by Dr. Bailey, with a view to its being used by a number of observers under defined and comparable conditions.

Dr. Bailey very kindly forwarded to Calcutta the details of the method he used, and a number of interesting comparative results were obtained. Other observations were also made, which, although necessarily of a frag-

* "The atmosphere of Manchester," G. H. Bailey *in* "Studies from the Physical and Chemical Laboratories of the Owens College," vol. 1, p. 251, 1893.

† *Proc. Roy. Soc.*, No. 203, 1880, p. 355. *16th Ann. Rept. of the Chief Inspector of Alkali Works*, 1879, p. 15.

‡ *17th Ann. Rept. of the Chief Inspector of Alkali Works*, 1880, p. 16.

mentary character, appear to be worth placing on record.

Description of Method used.

The method as described by Dr. Bailey is as follows:—

The following solutions are required:—

A. Potassium iodide (KI), 20 grams to litre.

B. Sulphuric acid (H_2SO_4), 11·85 grams to litre.

C. Sodium thiosulphate

12·7 c.c. to decolourise 10 c.c. D, *i.e.*,
1 c.c. = 1 mgm. iodine.

D. Centinormal iodine (1·27 grams. iodine dissolved in potassium iodide solution, and made up to 1 litre).

10 c.c. each of A and B are placed in a 2oz. bottle on a white porcelain plate 6 inches square, and completely exposed, *i.e.*, any obstruction x feet high must not be nearer than x yards from the bottle.

The records are ultimately calculated to indicate mgms. iodine liberated per 100 c.c. of solution A per hour.

Preliminary Experiments.

It was necessary in the first place to examine the influence of various factors which might be supposed to affect the method abnormally under the conditions met with in the tropics.

Effect of Temperature.

The first of these factors is evidently *temperature*, as this may easily rise in tropical sunlight to considerably over 100° Fahr.

It was found, however, that on keeping the ordinary mixture, made as above, for 12 hours in an incubator at 97° Fahr., only a negligible quantity of iodine (0·5 mgm.) was liberated. It may be concluded, therefore, that the reaction is practically unaffected by temperature under the ordinary conditions of exposure.

Effect of Duration of Exposure.

The next point to consider was the effect of variations in the *duration* of the exposure, and in this connection it was important to determine the effect of the liberated iodine in absorbing the chemically active rays.

For this purpose two bottles were exposed, one in the usual manner, and the other completely immersed in a centinormal solution of iodine contained in a small beaker. The following results were obtained :—

TABLE I.

Effect of Duration of Exposure.

Date.	Time.	Duration	c.c. Thio.		Mgms. Iodine per 100 c.c. per hour		Remarks.
1906.			Normal	Under Iodine.	Normal	Under Iodine.	
March 28	12-10 p.m.—3-10 p.m.	3 hrs.	0·7	0·2	2·3	0·7	Sky overcast
March 29	11-30 a.m.—3-30 p.m.	3 hrs.	6·1	1·6	20·3	5·3	Full sun
March 30	12 noon —1 p.m.	1 hr.	2·6	0·3	26·0	3·0	„ „
March 31	12 noon —1 p.m.	1 hr.	0·7	0·1	7·0	1·0	Half sun

The above results show clearly the absorbing effect of the iodine solution, and, in consequence, no doubt, of this, the rate of change during the three hours exposure on March 29th is appreciably less than during the one hour exposure on March 30th, both being in full sun. This is accentuated in an experiment made later, when the usual solution was exposed from 8-45 a.m. on April 18th to 6 p.m. on April 19th, a total of 33¼ hours comprising about 20 hours of full sun. In this case 16·5 c.c. thio were required corresponding to an average rate for the entire period of only 4·9 mgms. iodine per 100 c.c. per hour, or, assuming 20 hours sunshine, of 8·25 mgms. iodine per 100 c.c. per hour of sunshine, as against 26 mgms. on March 30th.

Effect of Light on Iodine Titrations.

The retarding effect of the presence of iodine on further change would indicate that but little alteration would take place in the short time required for titrating the iodine when carrying out "oxygen absorption" tests. At the same time it is obvious that solutions containing potassium iodide in presence of sulphuric acid should be kept as far as possible out of direct sunlight, and the titration of the iodine should take place without loss of time after the addition of potassium iodide to the mixture of permanganate and sulphuric acid. A possible loss of iodine may occur if the solution stands any length of time in an open vessel, owing to escape of iodine vapour at the high temperature of a tropical laboratory.

Effect of Dilution.

Finally, it was important to investigate the effect of *dilution*, as it might be assumed that the retarding effect of the liberated iodine would be less in dilute solution. Consequently two bottles were exposed, the one containing the normal mixture, the other the same mixture diluted with an equal volume (20 c.c.) of distilled water. The following results were obtained :—

TABLE II.

Effect of Dilution.

Date.	Time.	Duration	c.c. Thio.		Mgms. Iodine per 100 c.c. per hour.		Remarks.
1906.			Normal Solution.	Diluted Solution.	Normal Solution.	Diluted Solution.	
April 2 ...	1-10 p.m.—2-10 p.m.	1 hr.	2·7	1·6	27·0	16·0	Full sun
April 3 ...	11-30 a.m.—1-30 p.m.	2 hrs.	4·7	4·3	23·0	21·5	„ „
	1-50 p.m.—2.50 p.m.	1 hr.	2·4	1·7	24·0	17·0	„ „

It is evident that the initial reaction does not take place so rapidly in dilute solution, but that on longer exposure the two solutions tend to give results in closer agreement, owing, no doubt, to the greater retarding effect of the iodine liberated in the stronger solution. It will be seen also that very little difference in rate was observable between the 2 hour and 1 hour period on April 3rd, even with the solution of normal strength.

As a result of these preliminary experiments, a period of ½ hour to 1 hour was generally chosen for exposure, and the original mixture of 10 c.c. of sulphuric acid and 10 c.c. of potassium iodide solution was used as described above.

An experiment made later, see May 9th (Table III.), confirmed the above results in showing that the rate of liberation of iodine in the half hour or 1 hour period respectively varied but slightly.

Systematic Observations.

The method having been decided upon, a series of comparable observations of the chemical effect of bright sunlight were made in Calcutta and at various places between Calcutta and Marseilles. The object of the observations was in the first place, as already stated, to obtain some measure of the chemical intensity of sunlight in Calcutta, and in the second place to afford additional information to compare with the earlier observations collected by Dr. Bailey. It was further of interest to determine, if possible, whether any relation existed between the chemical effect of tropical sunlight and its liability to cause sunstroke or sunburn. The results of these observations are given in Table III.

For the purpose of carrying out the observations on the journey from Calcutta to Marseilles, a small box was

fitted up with bottles containing a sufficient quantity of the necessary reagents. The iodide and sulphuric acid were measured from a small graduated cylinder, and for the titrations a narrow graduated pipette, such as is used for bacteriological work, was employed. The whole kit thus occupied very little space. The thiosulphate solution being kept in the dark was found to alter only very slowly. It was retitrated against centinormal iodine on May 28th, after returning to Manchester. By that time a distinct deposit of sulphur had taken place, which was not noticed previously. Even then the correction required was not excessive. The results of the observations in the Mediterranean were adjusted accordingly, and the amended figures are given in brackets in the table. They are, however, certainly to be taken as minimum figures. A study of the table will show, in the first place, as might have been expected, that the chemical intensity is greatest in the middle of the day. It is also evident that the chemical activity is greater out at sea than inland, and that even in the Mediterranean higher results are obtained than in Calcutta or Agra.

Through the kindness of Dr. Bailey the writer is enabled, in Tables IV. and V., to include for comparison a number of hitherto unpublished records, chiefly obtained in Manchester, but also including observations made in other parts of England and the Continent.

The results set out in Table IV. are calculated in the same terms as those already given, and for the most part are taken under conditions of full sunlight, and are, therefore, comparable throughout. In the case of the results in Table V., with the exception of those at Pontresina, it is not quite certain whether there was full sun all the time.

TABLE III.

Systematic Observations.

Date.	Time.	Duration.	Place.	Lat.	Long.	C.C. Thio.	Mgms. I. per 100 c.c per hour.
1906. March 19	2-44 p.m.—3-12 p.m.	28 mins.	Calcutta (roof of Lab.)	22·5	88·5	0·9	19·3
	11-0 a.m.—12-0 noon	1 hr.	,,	,,	,,	3·0	30·0
	11-0 a.m.—1-0 p.m.	2 hrs.	,,	,,	,,	4·1	20·5
	11-0 a.m.—3-0 p.m.	3 hrs.	,,	,,	,,	5·4	18·0
March 22	11-0 a.m.—12-0 noon	1 hr.	,,	,,	,,	2·8	28·0
	12-15 p.m.—1-30 p.m.	1¼ hrs.	,,	,,	,,	2·7	21·6
	11-15 a m.—2-15 p.m.	3 hrs.	,,	,,	,,	7·0	23·3
	1·30 p.m.—2-30 p.m.	1 hr.	,,	,,	,,	2·8	28·0
March 30	12-0 noon —1-0 p.m.	1 hr.	,,	,,	,,	2·6	26·0
April 2	12-10 p.m.—1-10 p.m.	1 hr.	,,	,,	,,	2·9	29·0
	1-10 p.m.—2-10 p.m.	1 hr.	,,	,,	,,	2·7	27·0
	3-0 p.m.—4-15 p m.	1¼ hrs.	,,	,,	,,	2·0	16·0
April 3	11-30 a.m.—1-30 p.m.	2 hrs.	,,	,,	,,	4·7	23·5
	1-50 p.m.—2-50 p.m.	1 hr.	,,	,,	,,	2·4	24·0
April 17	8-45 a.m.—9-45 a.m.	1 hr.	,,	,,	,,	1·7	17·0
April 21	3-45 p.m.—4-45 p.m.	1 hr.	,,	,,	,,	1·6	16·0
April 27	3-40 p.m.—4-50 p.m.	1 hr. 10 mins.	,,	,,	,,	2·6	26·0
April 29	12-20 p.m.—1-20 p.m.	1 hr.	Verandah (Loudon Street).	,,	,,	2·2	22·0

TABLE III.—*Continued.*

Date.	Time.	Duration.	Place.	Lat.	Long.	C.C. Thio.	Mgms. I. per 100 c.c per hour.
1906.							
May 2	9-15 a.m.—10-15 a.m.	1 hr.	Agra (Hotel Compound)	27·0	78.5	2·4	24·0
	11-30 a.m.—1-0 p.m.	1½ hrs.	„	„	„	4·5?	30·0
	11-30 a.m.—1-0 p.m.	1½ hrs.	„	„	„	3·5	24·3
	3-45 p.m.—4-15 p.m.	½ hr.	„	„	„	1·2	24·0
May 5	2-20 p.m.—2-50 p.m.	½ hr.	Bombay Harbour.	19·0	72·5	1·4	28·0
May 7	2-5 p.m.—2-35 p.m.	½ hr.	Arabian Sea.	17·32 taken at	60·32 12 noon.	1·5	30·0
	3-20 p.m.—3-50 p.m.	½ hr.	„	„	„	1·5	30·0
	4-30 p.m.—5-0 p.m.	½ hr.	„	„	„	0·9	18·0
May 8	12-26 p.m.—12-56 p.m.	½ hr.	„	16·31	54·8	2·1	42·0
May 9	11-30 a.m.—12-0 noon.	½ hr.	„	13·59	48·37	1·7	34·0
	11-30 a.m.—12-30 p.m.	1 hr.	„	„	„	3·1	31·0
May 11	12-0 noon—12-30 p.m.	½ hr.	Red Sea.	16·57	40·51	1·7	34·0
May 13	2-15 p.m.—2-45 p.m.	½ hr.	Gulf of Suez.	27·27	34·04	1·6	32·0
May 15	11-30 a.m.—12-0 noon.	½ hr.	Mediterranean.	32·45	29·28	1·5 (1·32)	30·0 (26·4)
May 16	1-10 p.m.—1-40 p.m.	½ hr.	„	35·22	22·58	1·9 (1·67)	38·0 (33·4)
May 18	11-50 a.m.—12-20 p.m.	½ hr.	„ „ (full sun ⅔ of time)	41·01	10·17	1·5 (1·32)	30·0 (26·4)

TABLE IV.

Observations in Manchester.

Exposures, 11 a.m.—1 p.m. All during bright sunshine. Results stated in Mgms. I. per hour.		
September 3, 1891.....	York Place	13·5
October 12, ,, ...	,, ,, ...	8·0
December 17, ,, ...	,, ,, ...	1·0
,, 27, ,, ...	Didsbury	1·2
January 1, 1892.....	,, ...	3·0
,, 16, ,, ...	,, ...	2·75
,, 16, ,, ...	Longsight	1·0
,, 24, ,, ...	Didsbury	5·0
,, 31, ,, ...	,, ...	3·75
,, 31, ,, ...	York Place	1·1
February 18, ,, ...	,, ,, ...	2·2
,, 23, ,, ...	,, ,, ...	4·5
,, 25, ,, ...	,, ,, ...	8·0
March 19, ,, ...	,, ,, ...	8·0
,, 19, ,, ...	Didsbury	9·8
April 3, ,, ...	,, ...	10·0
May 1, ,, ...	,, ...	12·0
,, 1, ,, ...	York Place	11·4
,, 9, ,, ...	,, ,, ...	9·5
,, 9, ,, ...	Didsbury	13·0
June 9, ,, ...	,, ...	14·0
,, 9, ,, ...	York Place	13·0

TABLE V.

Miscellaneous Observations.

1 HOUR EXPOSURE AT MID-DAY.

Date.	Place.	Latitude.	Longitude.	Mgms. I. per hour.
—	Grindelwald	46·5	8 (E)	20·0
December 27	Didsbury	53·5	2 (W)	4·7
December 27	Barnard Castle	54·5	2 (W)	3·5
December 27	Worcester	52·2	2 (W)	3·5
January 1 ...	Malvern.....	52	2 (W)	4·4
January 1 ...	Malvern Hills	52	2 (W)	6·0
January 1 ...	Torquay.....	50·5	3·5 (W)	11·0
August 18, 1892	Pontresina, 6,000 feet	46·5	10 (E)	26·5
August 29, 1892	Pontresina, 10,000 feet	,,	,,	30·2

It will be seen that the records for Manchester range from 1 on a sunny day in December to 14 on a day in June.

The highest record in Table III. was obtained on May 8th, 1906, in the Arabian Sea (latitude, 16·31; longitude, 54·8), in the vicinity of the Arabian coast. This is 42 times the lowest December record for Manchester. *i.e.*, in one day in the Arabian Sea there is as much actinic sunlight as reaches Manchester in 6 weeks of wintry sunshine. The highest summer record for Manchester is 14, or one-third of the Arabian Sea maximum. A normal reading for Calcutta may be taken as 26, or nearly double the highest for Manchester. An

isolated record made by the author last September at York Place gave 18, appreciably higher than Dr. Bailey's maximum, but such a result is evidently exceptional. These great differences in the amount of actinic light must, one would think, correspondingly affect the human organism. One of the most depressing features of life in Manchester and other large towns is the absence of light.

One result from Pontresina is actually higher than any obtained in Calcutta. Dr. Bailey suggests that the high results here may be due to the high temperature, but the highest shade temperature he gives for Pontresina is 86° Fahr., while in April in Calcutta it was seldom below 98° Fahr. and often over 100°. Moreover it has already been shown that the effect is practically independent of temperature. It is quite likely, however, as Dr. Bailey also suggests, that the great height of the station, viz., 10,000 feet, will be of influence, owing to the diminished atmospheric absorption. In fact the two results, taken respectively at 6,000 and 10,000 feet, would bear this out.

Possible Relation between Photo-chemical Activity and Sunstroke.

It has been held that the sunstroke effect is due to the actinic rays, and it is stated by Colonel Maude* that by protecting the head and neck with a red screen, on the analogy of the photographer's red lamp, he has been able completely to withstand the effect of sun, even in the hottest parts of India.

The Pontresina results are specially interesting in reference to the possible relation between the chemical intensity of the light and its power to cause sunstroke. It is universally considered unsafe to be out in the sun during the day in India, without a properly constructed

* "War and the World's Life," p. 364.

pith helmet. This is also the case in the hills, where the temperature may be low enough to necessitate European winter garments. In the Arabian sea cases of sunstroke are reported as occurring through inadvertent exposure for a very short time outside the deck awnings. In the Mediterranean, however, such protection is considered unnecessary, although, as already stated, the records in the Mediterranean are higher than in Calcutta.

This would lead to the conclusion, that the sunstroke effect does not depend upon photo-chemical rays.

This conclusion is further confirmed by some experiments, facilities for which were kindly afforded by Captain Llewellyn of R.M.S. "Oriental" while crossing the Arabian Sea. Captain Llewellyn stated that frequent cases of sunstroke occurred among crews in open boats in the tropics, and the question arose whether the effect was due in any measure to rays reflected from the water. In order to determine whether the photo-chemical effect was increased by such rays, one bottle was slung overside and suspended about one foot from the water, another was exposed in the usual way on a white plate, and a third on the planking of the deck. The observation was taken on May 8th, 1906, from 12-26 to 12-56 p.m., lat. 16·31, long. 54·8. The Arabian coast was in sight, the sunshine was brilliant, and the sea a bright blue. The following results were obtained :—

Position of Bottle.	c.c. Thio.	Mgm Iodine per 100 c.c. per hour.
Overside	1·7	34·0
On deck	1·7	34·0
On white plate	2·1	42·0

On the following day (May 9th, 1906) a further experiment was made, the second bottle in this case being suspended one inch above the surface of the water in a large open tub coated inside with white enamel, the control being on a white plate as usual, and both in full sun. The period of observation was from 11-30 a.m. to 12 noon. The position of the steamer was about lat. 13·59, long. 48·37, some two miles from the Arabian coast. The control again gave the higher result, viz., 34 mgms. iodine per 100 c.c. per hour as against 22 mgms. in the case of the bottle suspended over the water.

It may be concluded, therefore, that the chemical rays are not reflected from the surface of the water to the same extent as from such a surface as is presented by white porcelain, and to no greater extent than from the ordinary white planking of the deck.

From the results which are here recorded, it would thus appear that no relation exists between the photochemical and the sunstroke effect of sunlight. This is confirmed by the fact, that pith helmets are considered equally necessary in India on dull or bright days. The author was informed that many sunstrokes occur on dull days in Calcutta through carelessness in uncovering the head.

On the other hand the effect can hardly be one of temperature, as "topis" have to be worn both in the hot and cold weather in India, and also, as has been stated, in the hills. Moreover they are not worn in Melbourne, where the temperature may often be as high as in India. It is, of course, possible that rays other than either the actinic or the ultra red rays may have to do with the phenomenon of sunstroke.

It has further been supposed that the effect depends on the verticality of the sun's rays, and consequently, on

geographical position in reference to the equator. The author is not aware how far this is borne out by experience in other tropical countries. It is not quite easy to see, if the sunstroke effect is neither a heat effect nor a photo-chemical effect, why it should depend on the verticality of the rays. The question may perhaps be left to the physicists and medical men to discuss.

There would appear to be some relation between photo-chemical activity and *sunburn.* A very short inadvertent exposure of a lady's short sleeved arm on the gunwale of the tender, while changing steamers at Aden, caused considerable reddening and final peeling of the skin, while the often rather serious effects of sunburn in the Alps are familiar. In this latter case temperature can hardly be a factor. On the other hand, the author has spent a good many days working out of shelter in full sun in the neighbourhood of Calcutta, without being as sunburnt as after a few days by the sea in this country. As in the case of sunstroke, some other factors than photo-chemical activity and temperature would appear to be concerned with the phenomenon of sunburn.

The Absorption of Photo-chemical Rays.

The absorbing effect of iodine solutions has already been referred to. Dr. Bailey remarks (*loc. cit.* p. 253) that the photo-chemical rays are almost completely absorbed by a solution of bichromate of potash and by chlorophyll.

A few experiments were made on the absorbing effect of various solutions, by exposing the standard mixture to light in a narrow test-tube placed within a larger one, the absorbing solution being contained in the annular space between the two test-tubes. The arrangement

afforded a layer of absorbing solution of approximately one millimetre in thickness. The volumes of mixture taken were measured and results were calculated on the usual basis.

The following results may be quoted :—

April 24th, 1906.—Full sun on roof of laboratory, 9-10 a.m.	
Absorbing Solution.	Mgms. Iodine per 100 c.c. per hour.
5% $K_2Cr_2O_7$	5
Chlorophyll [100 c.c. solution gave ·087 grm. residue.]	2
2% $CuSO_4$ + excess NH_4OH	18
Water	23
Normal Mixture as control	24

April 27th, 1906.—Fairly constant sun on roof of laboratory, 3-50—5 p.m.	
Absorbing Solution.	Mgms. Iodine per 100 c.c. per hour.
5% $K_2Cr_2O_7$	Trace
Chlorophyll as above	Trace
$\frac{N}{100}$ Iodine	2·2
5% $CuSO_4$ + equal vol. 25% NH_4OH (·88 s g.)	16·5
Water	17 0

These results confirm Dr. Bailey's observations, even with stronger sunlight and very slight depth of absorbing solution. The differences between the absorbents are accentuated with the stronger sunlight, the absorbing

effect of chlorophyll even in dilute solution is extraordinary. If the sunstroke effect were dependent upon the photo-chemical effect a very thin layer of chlorophyll containing material should be a perfect protection.

The Effect of Sunlight on Chloride of Lime Solutions.

In considering the effect of cloride of lime as a sterilising agent for sewage effluents, it is important to investigate the stability of its solutions under various conditions.

It was found that dilute solutions of chloride of lime lose their available chlorine with extraordinary rapidity on exposure to Calcutta sunlight.

Thus a solution of chloride of lime containing 45·5 milligrams available chlorine per 100 c.c., lost 18 milligrams or nearly 40 per cent. of the available chlorine on exposure for one hour on April 21st, from 3-45 to 4-45 p.m., a day when, as can be seen from Table III., the chemical activity of the sunlight was only about two-thirds of its maximum.

On April 27th, when a maximum reading, viz., 26·0, was obtained by the iodine test, a solution of chloride of lime, containing 85 mgms. chlorine per 100 c.c., lost 34·6 mgms. or only slightly over 40 per cent. on one hour's exposure, indicating that the tendency to de-composition decreased with increase of strength of the solution. The same solution on keeping for one hour in the dark, and on being held for 10 minutes in boiling water, suffered no decrease in strength.

These experiments were subseqently confirmed by Captain, now Major Clemesha, Deputy Sanitary Commissioner for Bengal.

The following table summarises his results.

No. of Experiment.	Time of Exposure.	Mgms. Cl. in 100 c.c.	Percentage Loss.
1	0	1·52	
	1 hour	nil	(100 practically)
2	0	61·60	
	1 hour	36·3	41
	2 hours	7·9	87·1
	3 hours	2·2	96·4
3	0	850	
	2 hours	797	6·25
	4 hours	791	9·3
	6 hours	750	11·8
	8 hours	713	16·1

The above results show clearly the much less percentage loss which occurs with increased concentration of the solution. Major Clemesha further showed that little or no loss occurred in the dark or at the temperature of boiling water.

Professor Delépine informs the writer that he has also noticed the deteriorating effect of sunlight on solutions of chloride of lime in this country and the variation of the effect with concentration.

The importance of these results in connection with sterilisation or disinfection by chloride of lime is obvious.

The precise character of the change which chloride of lime solutions undergo in presence of light was not studied, but, in view of the experiments of Pedler* and of Richardson†, it would appear most probable that calcium chlorate is formed.

The Effect of Sunlight on Chloride of Silver, etc.

It was thought possible that, in the intensely active sunlight of Calcutta, chloride of silver and other substances might suffer molecular change to a greater degree than had hitherto been observed. Accordingly an old experiment suggested by Abney was repeated. Well washed silver chloride was dried on the water-bath and sealed up in a glass tube, in one case alone, in the other case with a small globule of mercury, and exposed to light. Darkening took place in both cases, and on placing in darkness, the silver chloride in the tube without mercury became completely white again, while in presence of mercury the darkening persisted. This difference in effect appeared to continue indefinitely, however long the exposure to light, or however frequent the intervals of darkness. It would appear, therefore, that light is incapable of so far modifying the chloride of silver as to render it incapable of taking up in the dark the chlorine lost in the light, while on the other hand, in presence of mercury, it was not possible to say that a definite limit to decomposition had been reached. These results would favour the supposition that the darkening of silver chloride in light is due to a simple dissociation effect, rather than to the formation of an oxychloride.

* "The Action of Chlorine on Water in the Light, and the Action of Light on certain Chlorine Acids." Alexander Pedler, F.I.C., *Journ. Chem. Soc. Trans.* LVII., 1890, p. 613.

† "The Reaction between Calcium Carbonate and Chlorine Water." Arthur Richardson, Ph.D., *Journ Chem. Soc. Trans.* XCIII., 1908, p. 280.

Red lead, Pb_3O_4 and *yellow oxide of mercury*, HgO were exposed to light in sealed tubes, but no well-defined change took place.

A selection of different coloured silk ribbons, such as are ordinarily sold in Calcutta, was exposed to light for varying periods. It was found that light green ribbon rapidly faded, the effect being appreciable after a few hours bright sunshine, light blue was also very sensitive, orange yellow darkened somewhat on exposure, while red altered only very slowly.

Summing up the results of the scattered observations in this paper, it may be said that the photo-chemical effects of tropical sunlight do not differ in kind from those observed under European conditions, and that even in certain favoured European localities equally striking effects may be obtained.

The method of observation introduced by Dr. Angus Smith and employed as suggested by Dr. Bailey, has proved simple to carry out and trustworthy in its results, and it may be hoped that other observers will take up the subject as opportunity offers, and thus extend our quantitative knowledge of photo-chemical activity in various parts of the world.

Manchester Memoirs, Vol. lii. (1908), *No.* **15.**

XV. An Annotated List of the Alien Plants of the Warrington District.

By G. A. DUNLOP.

Received and read March 24th, 1908.

The Warrington District may be defined as the area included within a radius of ten miles from Warrington Bridge.

During the last few years the number of adventitious plants has been increasing to such an extent that it is desirable that some record should be made of their stations and habitats, and, where possible, of their origin, while the data are still fresh.

As many of these aliens depend for their introduction upon commercial, industrial, and domestic operations, the means of introduction may be summarised as follows:—

1. The construction of the Manchester Ship Canal, 1887-1894; and the opening of the same to traffic from all parts of the world.
2. During the last twelve months the railway between Warrington and Acton Grange High Level Bridge has been doubled, and many plants entirely new to the local flora have been introduced in the imported soil used in making the new embankments.
3. In two comparatively small areas, quite an alien flora has sprung up owing to the use of corn siftings for feeding poultry.

July 22nd, 1908.

Myrrhis Odorata, Scop. Denizen.

58. Thelwall, beside the old river course. Thelwall, on the banks of the Mersey, near the Ship Canal ferry.

59. Appeared for years in the garden of Stanley House, Bold Str., Warrington. (C.R.B.). Padgate, railway embankment; also at intervals as far as Urmston. (C.R.B.)

**Archangelica officinalis*, Hoffm. Alien.

59. One huge plant occured in Arpley, about 100 yards below the railway bridge in 1887. It grew on the mud bank close to the water's edge. (C.R.B.)

**Coriandrum sativum*, Linn. Alien.

58. Acton Grange, railway embankment, Sept., 1907; one fine plant.

**Galium Mollugo*, Linn. Alien.

59. Orford, railway embankment, 26th July, 1896. (C.R.B.)

Dipsacus sylvestris, Huds. Native.

58. Aston, banks of Weaver. This is J. F. Robinson's old station.

59. Hale. (C.R.B.). Mentioned as "casual here" in "Cheshire Flora." Mentioned in "Manchester Flora" as occuring in Warrington. There is no evidence of it in the district now.

**Erigeron canadense*, Linn. Alien.

58. Acton Grange, railway embankment, July, 1907. Crowton, in hedgebank, on the road to Acton, 5th Aug., 1907.

**Ambrosia artemisifolia*, Linn. Alien.

58. On the north bank of the Ship Canal at the east end of Latchford Locks. The plant is quickly establishing

itself here, and during the last three years has taken possession of a deal of ground. In this station the plants are dwarfish with short dense foliage. I have not been able to trace the origin of the plant here, it had evidently been here for some years previous to its discovery.

Latchford, on the site of the old railway station. The species is an undoubted importation here, having been introduced with grain siftings. The plants growing in this station are somewhat different in habit from those at the Locks. They are tall—nearly two feet in height—diffuse in habit, and the leaves are much longer with the segments longer and narrower.

**Ambrosia trifida*, Linn. Alien.

58. Another "hen corn" import. Latchford, on the site of the old railway station, July, 1907. A number of plants growing on the ground where the poultry are fed.

**Helianthus annuus*, Linn. Alien.

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

**Helianthus tuberosus*, Linn. Alien.

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Anthemis nobilis, Linn. Denizen and alien.

58. Acton Grange, railway embankment, Sept., 1907; alien.

59. Hale; John Peers, in *The Phytologist*, 1863. Here in 1887. (C.R.B.). Recorded for vice-county 59, in "Top. Bot.," ed. ii., p. 261.

**Artemisia Absinthium*, Linn. Alien.

58. Warburton, August, 1888. Waste ground on Morley Common, 27th Aug., 1891. (C.R.B.). Banks of Ship Canal, Acton Grange, growing near to the water edge, July, 1905. Recorded for Co. 58, by Warren, in "Top. Bot.," ed. ii, p. 251.

**Senecio viscosus*, Linn. Alien.

59. Banks of "Black Bear" Canal, near Latchford Bridge, June, 1905.

Senecio sarracenicus, Linn. Formerly denizen, but now extinct.

The "Flora of Cheshire," gives the following Warrington stations for the species.

58. Lymm, 1868; Statham Eye, below Lymm, 1859.

59. Opposite Woolston and Bruch.

Mersey bank, close to Warrington, opposite Latchford.

NOTE.—"The plant had disappeared previous to 1886. I often searched in vain for it in the district." (C.R.B.)

The disappearance of the Warrington plants may probably be explained by the abundance of specimens in the Wilson Herbarium in the Warrington Museum!

Carlina vulgaris, Linn. Alien.

58. Latchford, near site of old railway station, 15th September, 1907. Introduced with grain siftings. Recorded by Warren for county 58, in "Top. Bot.," ed. ii., p. 246.

Carduus acanthoides, Linn. Alien or denizen.

58. Weaver Bank, near Aston; and at Sutton Lock, 30th August, 1891; denizen. (C.R.B.)

59. Slutcher's Lane. (C.R.B.). Casual. Walton Arches. (C.R.B.)

**Cnicus eriophorus*, Roth. Casual.

58. Grappenhall, on waste ground, Sept., 1907.

Silybum Marianum, Gærtn., = *Mariana lactea*, Hill. Garden escape and alien.

58 Cliffe Lane, Grappenhall, August, 1906.

59. Sankey; John Peers in *The Phytologist*, 1863. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Centaurea Scabiosa, Linn. Alien.

59. Orford, railway bank, 26th July, 1896. (C.R.B.). Recorded for vice-county 59 in "Top. Bot.," ed. ii., p. 247.

Centaurea Cyanus, Linn. Casual; frequent throughout the district.

**Centaurea melitensis*, Linn. Alien.

58. Latchford, near old railway station site, Oct., 1907. Introduced with corn siftings. Plentiful.

**Centaurea solstitialis*, Linn. Alien.

59. Slutcher's Lane, 10th July, 1891. (C.R.B.). Waste ground, Arpley, August, 1906.

**Carthamus tinctorius*, Linn. Alien.

58. Grappenhall, on waste ground, Sept., 1907.

Cichorium Intybus, Linn. Alien.

Chicory is becoming a frequent plant in the district, near the tracks of the railways and canals.

Picris echioides, Linn.,= *Helmimthia echioides*, Gært. Native.

58. Banks of the Weaver Canal at Sutton Lock, and at intervals thence up the river towards Dutton Viaduct, 1887. (C.R.B.). Still in the Sutton Lock station in 1906.

NOTE.—These stations for the plant are not given in the "Flora of Cheshire." See p. 188.

59. Hale; John Peers, in *The Phytologist*, 1863.

**Hieracium aurantiacum*, Linn. Alien.

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Campanula rapunculoides, Linn. Denizen.

58. Crowton, hedgebottom, July, 1907.

59. Bickerstaffe, hedgebank, August, 1907.

NOTE.—This locality is a little outside our district, but as the plant is not recorded from this station in the "Liverpool Flora," and as it was brought to me for identification, I place it on record here.

Campanula sp., non. det. Alien.

58. Latchford, on the site of the old railway station; an introduction with grain siftings; two plants.

NOTE.—The plants evidently have had a hard struggle for existence, and are not sufficiently developed for identification. The foliage does not answer to that of any British species.

**Kalmia angustifolia*, Linn. Alien.

59. Rixton Moss, planted? Been established for many years.

Vinca minor, Linn. Denizen.

58. Appleton Denna. Thelwall, in a thicket.

59. Hale, in a plantation, 1886. (C.R.B.)

**Cynoglossum officinale*, Linn. Native? or denizen.

58. Roadside, near Hatchmere, on the road to Manley. 1896. "A few very fine plants. This is the only inland locality in Cheshire in which I have seen it." (C.R.B.). Recorded for county 58 by Warren in "Top. Bot.," ed. ii., p. 329.

**Amsinckia lycopsoides*, Lehm. Alien.

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

**Borago officinalis*, Linn. Alien.

58. Grappenhall, banks of Bridgewater Canal, garden outcast. Acton Grange, railway embankment, July, 1907.

59. Burtonwood, John Peers in *The Phytologist*, 1863. Hale, 1887. (C.R.B.)

Anchusa officinalis, L. Alien.

59. Arpley, 13th June, 1857. "In the luggage yard, Arpley Station, evidently introduced with some ballast." (Label on specimen in Warrington Museum herbarium). Now extinct.

Anchusa sempervirens, Linn. Alien.

58. Thelwall, in private grounds near the church, introduced with poultry corn.

59. Arpley Station, Sept., 1862. Specimen in Warrington Museum herbarium. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Lithospermum arvense, Linn. Alien.

59. Orford; John Peers in *The Phytologist*, 1863.

Echium vulgare, Linn. Alien.

58. Latchford, on site of old railway station: introduced with foreign grain siftings.

59. Arpley Station, 19th June, 1856. "In some ballast brought from the Yorkshire coast." (Label on specimen in the Warrington Museum herbarium). Now extinct. Orford, railway bank., 26th July, 1896. (C.R.B.)

Solanum nigrum, Linn. Denizen and casual.

58. Stockton Heath, in profusion in a field behind the church, and spreading into the neighbouring sandpit; Stockton Heath, in field near Lumb Brook.

59. Garden weed in Wilson-Patten Street, Warrington. (C.R.B.)

**Physalis Alkekengi*, Linn. Alien.

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Lycium chinense, Mill., = *L. barbarum*, auct. Denizen; at cottage doors, and in gardens and garden hedges, also extending along hedgerows.

Datura Stramonium, Linn. Alien.

58. Weaver bank, above Sutton Lock. (C.R.B.)

59. Arpley stone-yard, 30th July, 1891. (C.R.B.)

Hyoscyamus niger, Linn. Alien.

58. Weaver bank, near Sutton Lock, 30th Aug., 1896. (C.R.B.)

59. Slutcher's Lane, 30th July, 1891. (C.R.B.)

Verbascum Thapsus, Linn. Denizen and alien.

58. On the banks of the L. & N. W. Railway main line, immediately after it passes under the Chester line south of Moore and Daresbury. It grew here sparingly: there in 1900. (C.R.B.). Moore, spoil bank near swing bridge. Alien. Thelwall, planted by the late Thos. Glazebrook Rylands.

59. Hale; John Peers in *The Phytologist*, 1863. Slutcher's Lane, 30th July, 1891. (C.R.B.). Alien.

**Calceolaria mexicana*, Benth. Alien.

58. Thelwall, growing on ground where poultry have been fed with foreign grain siftings, August, 1907.

NOTE.—"This native of Mexico is growing and thriving, and apparently in parts self-sown, in the Rock Garden, at Kew, and seems quite at home in the open." (Extract from a letter from W. S. Laverock, M.A., B.Sc., of Liverpool Museum, who identified the plant for me while on a visit to Kew Gardens.)

Linaria Cymbalaria, Mill. Denizen.

58. Grappenhall, on an old stone wall.

59. Alder Lane, on an old brick bridge near Orford Hall. Bank Park, growing on the walls of the Vivarium.

**Linaria minor*, Desf. = *L. viscida*, Moench. Casual.

58. Hill Cliffe, on side of footpath near Birchdale. Acton Grange, railway embankment on newly tipped rubbish, July, 1907.

**Mimulus moschatus*, Doug. Escape.

58. Along both sides of a stream between Pettypool and the Over road, 15th Aug., 1891. In good quantity. (C.R.B.)

Veronica didyma, Ten. Native?

59. Padgate, 1887. (Jos. J. Smith).

Veronica Tournefortii, C. C. Gmel. Denizen.

59. Padgate, 1887. A garden weed. (Jos. J. Smith).

**Salvia verticillata*, L. Alien.

58. Latchford, almost under the cantilever bridge.

**Salvia officinalis*, Linn. Alien.

59. Orford, railway bank, 26th July, 1896. (C.R.B.)

Nepeta Cataria, Linn. Alien.

59. Orford, railway bank, 26th July, 1896. (C.R.B.)

Marrubium vulgare, Linn. Alien.

58. Walton Pond and Morley Common, 9th July, 1891. (C.R.B.). Warren records it for county 58 in "Top. Bot.," ed. ii., p. 320.

Galeopsis Ladanum, Linn. Denizen and colonist; not uncommon at the borders of fields and hedge bottoms near cultivated land.

**Galeopsis dubia*, Leers. Alien.

58. Acton Grange, in a potato field between the Ship Canal and the railway.

NOTE.—A specimen was sent to Dr. Eric Drabble, of Liverpool University, and identified by him. Vide *Journal of Botany*, vol. 44 (1906), p. 396.

Galeopsis speciosa, Mill, = *G. versicolor*, Curt. Denizen or colonist? frequent; has a preference for potato crops.

**Leonurus Cardiaca*, Linn. Garden outcast.

58. Grappenhall, July, 1906, and again in 1907.

Lamium hybridum, Vill. Denizen and alien.

58. Grappenhall, growing plentifully on a hedgebank in a cultivated field; had every appearance of having been there for years, June, 1906. Acton Grange, railway embankment, July, 1907; alien.

**Lamium maculatum*, Linn. Garden outcast.

58. Grappenhall, amongst garden rubbish on the banks of the Bridgewater Canal, near the old vicarage.

Herniaria hirsuta, Linn. Alien.

58. Thelwall. Two plants appeared in different parts of a kitchen garden. Identified by Dr. Drabble. One specimen has been placed in the Warrington Museum herbarium, and the other in the National Herbarium at the British Museum. Vide *Journal of Botany*, vol. 44 (1906), pp. 395-6.

**Chenopodium serotinum*, Linn., = *C. ficifolium*, Sm. Alien.

58. Thelwall, on roadside opposite station goods yard, July, 1906. Again in same place, 1907. Identification confirmed by Mr. Charles Bailey.

**Chenopodium murale*, Linn. Denizen.

59. Fiddler's Ferry, 15th Aug., 1893. Fine plants in a barge-building yard, near the river, and extending some way along the canal. (C.R.B.). Recorded for vice-county 59 in "Top. Bot.," ed ii., p. 346.

Chenopodium rubrum, Linn. Denizen? becoming rather plentiful in the district on rubbish tips, and also on canal banks where barges formerly discharged manure.

Var. pseudo-botryoides, H. C. Wats.

58. Thelwall, on canal bank, near station. Grappenhall, on banks of Bridgewater Canal.

**Fagopyrum sagittatum*, Gilib.,=*F. esculentum*, Moench. Alien.

58. Acker's Pits, Stockton Heath, 28th Aug., 1905. Hill Cliffe, 1857.

Daphne Mezereum, Linn. Garden outcast.

58. Grappenhall.

Euphorbia exigua, Linn. Colonist?

59 Burtonwood; John Peers in *The Phytologist*, 1863.

Euphorbia Lathyrus, Linn. Casual.

58. Thelwall, appeared as a garden weed in 1905, has increased in 1907.

59. It appeared in the garden of Stanley House, Bold Street, Warrington, for a year only, 1888. (C.R.B.)

**Cannabis sativa*, Linn. Alien.

58. Banks of Mersey at Thelwall, 1890. (C.R.B.)

Parietaria ramiflora, Mœnch.,=*P. officinalis*, Linn. Denizen.

58. Thelwall. Hartford, in a cottage wall near the station. (C.R.B.). Hedge cop at the corner of the Whitegate Road, 21st July, 1897. (C.R.B.)

59. Winwick stone quarry, side nearest the road. C.R.B.)

Stratiotes Aloides, Linn. Not native, planted.

58. Latchford, planted; John Peers, in *The Phytologist*, 1863.

NOTE.—This station refers to the pond at Thelwall Lane, from whence a specimen was taken by Mr. Peers and planted in a pond near Moss Wood, Acton Grange. Both

stations have now been destroyed. Mr. James Mort, of Lymm, informed me that the late Mr. Thomas Glazebrook Rylands was responsible for the introduction of the plant into the Thelwall Lane pond, and after he built "Highfields," his house at Thelwall, he took some plants from this pond and transferred them to an ornamental pond in his grounds, where I saw several fine plants in July, 1907.

Crocus nudiflorus, Sm. Denizen.

58. Meadow at Latchford, 16th October, 1899. (C.R.B.). Grappenhall, "teste Mr. Pemberton." (C.R.B.)

59. Howley Fields. (Jos. J. Smith).

NOTE.—I have heard of this plant from various places along the Mersey banks, but have not seen specimens.

Narcissus Pseudo-Narcissus, Linn. Native or denizen?

58. In a meadow near Thelwall; denizen.

59. On an old garden site near Enfield Hall. Croft. Sankey, along Whittle Brook. (C.R.B.)

Galanthus nivalis, Linn. An escape.

58. Lower Walton, banks of Mersey. (C.R.B.)

Leucojum æstivum, Linn. An escape.

58. Stretton, in a hedge cop, July, 1886. (C.R.B.)

Ornithogalum umbellatum, Linn. Garden escape.

58. Railway bank at Frodsham. (C.R.B.)

Acorus Calamus, Linn. Denizen.

59. Longford meadows; John Peers in *The Phytologist*, 1863. Ditch near Alder Lane, leading to Longford bridge, 9th September, 1890. (C.R.B.). I could not find the plant in these stations in 1907.

Setaria viridis, Beauv. Alien.

58. Canal bank, below Daresbury Firs, 11th Sept., 1893. (C.R.B.)

Phalaris canariensis, Linn. Casual.

Very frequent, but always on rubbish heaps and waste ground.

**Bromus erectus*, Huds. Alien.

58. Walton, spoil bank, 10th June, 1896. (C.R.B.)

**Bromus secalinus*, Linn. Alien.

58. Walton, spoil bank, 10th June, 1896. (C.R.B.)

**Bromus arvensis*, Linn. Alien.

58. Walton, spoil bank, 10th June, 1896. (C.R.B.)

NOTE.—"Two other species of *Bromus* occurred in the medley of alien grasses that appeared in this station, neither of these are in the London Catalogue and they were not identified. (C.R.B.)

Lolium temulentum, Linn., and var., *arvense*, With. Aliens.

58. In the greatest profusion on the spoil bank at Walton Arches.

Secale cereale, Linn. Casual; occasionally on waste ground.

Hordeum murinum, Linn. Alien.

58. Preston Brook, on canal side, 3rd July, 1896. (C.R.B.). Acton Grange, railway embankment, Sept. 1907.

59. Orford, railway bank, 11th June, 1888. (C.R.B.)

Onoclea sensibilis, Linn. An escape.

58. "Near Warrington: no doubt an escape." Hooker's "Student's Flora," p. 535.

NOTE.—The "near Warrington" station of Hooker is Orford. The plant is locally known as the "Orford Fern." The earliest mention of it is found in "A Catalogue of the Plants in the garden of John Blackburne, Esq., at Orford Hall, Lancashire," by Adam Neale, gardener, and published by William Eyres at the Warrington Press in 1779.

The fern still occurs sparingly in the neighbourhood.

PROCEEDINGS

OF

THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1st, 1907.

Professor H. B. Dixon, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors ot the books upon the table. The following were among the recent accessions to the Society's Library: "*Reports of the Commission... for the Investigation of Mediterranean Fever*," Pts. 6, 7 (8vo., London, 1907), presented by the Royal Society of London; "*Handbook of American Indians, North of Mexico*," edited by F. W. Hodge, Part I. (8vo, Washington, 1907), presented by the Bureau of American Ethnology; "*Catalogue of the Gleave Brontë Collection at Moss Side Free Library*," by J. A. Green (8vo., Moss Side, 1907), presented by the Manchester Public Library; "*Astrographic Catalogue 1900-0. Oxford section Dec.* + *24° to* + *32°* "...[prepared] under the direction of H. H. Turner... vol. 2 (4to., Edinburgh, 1906), presented by the University Observatory of Oxford; "*The Licensed Trade*," by E. A. Pratt (8vo., London, 1907), presented by the author; "*Collected Mathematical Works of G. W. Hill*," vol. 4 (4to., Washington, 1907), presented by the Carnegie Institution; "*Balch Genealogica*," by T. W. Balch (4to., Philadelphia, 1907), presented by the author; "*Festschrift Theodor Curtius zum 25 jährigen Doktor-Jubiläum gewidmet.*" Arbeiten...von G. Bradig [and others] (8vo., Heidelberg, 1907), presented by the Chemisches Institut der Univ. Heidelberg; "*Flora Capensis*," ed. by Sir

W. T. Thiselton-Dyer, vol. 4, sect. 1, parts 2, 3. (8vo, London, 1905 and 1907), purchased; "*The Adventures of Hadji Baba of Ispahan*," [by J. Morier] transl. from English into Persian (8vo., Calcutta, 1905), presented by the Asiatic Society of Bengal; "*The Phalloids of Australasia*," (8vo., Cincinnati, 1907), "*The Nidulariaceae or 'Bird's-nest Fungi*,'" (8vo., Cincinnati, 1906), "*The Tylostomeae*" (8vo, Cincinnati, 1906), by C. G. Lloyd, presented by the Lloyd Library of Cincinnati; "*Le Opere di Galileo Galilei*," ed. naz., vol. iii., parte 2 and vol. xix. (4to, Firenze, 1907), "*Trent' anni di Studi Galileiani*," by A. Favaro (4to., Firenze, 1907), presented by the Italian Embassy; "*The Commercial Possibilities of West Africa*," by Viscount Mountmorres (8vo, Liverpool, 1907), presented by the Liverpool University Institute of Commercial Research in the Tropics.

The SECRETARY exhibited to the members an engraved portrait of John Dalton, a lock of Dalton's hair, and a portrait of Thomas Young, formerly Secretary of the Royal Society, engraved by Adcock, from the painting by Sir Thomas Lawrence, which were presented to the Society by Dr. F. W. Jordan.

It was resolved that the thanks of the Society be accorded to Dr. Jordan for his interesting gift.

The PRESIDENT then delivered his **Inaugural Address.**

The Address is published in full in the "Memoirs."

General Meeting, October 15th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Mr. ERNEST RUTHERFORD, M.A., D.Sc., F.R.S., Langworthy Professor of Physics in the University of Manchester, *17, Wilmslow Road, Withington, Manchester;* Mr. GEORGE H. WINSTANLEY, F.G.S., M.I.M.E., Lecturer in Mining Engineering and Mine Surveying in the University of Manchester; Mr. H. GEORGE A. HICKLING, B.Sc., Assistant-Lecturer and

Demonstrator in Geology in the University of Manchester; Mr. F. H. GRAVELY, B.Sc., Assistant-Lecturer and Demonstrator in Zoology in the University of Manchester; Mr. J. L. SIMONSEN, M.Sc., Assistant-Lecturer in Chemistry in the University of Manchester, *152, Barlow Moor Road, West Didsbury, Manchester;* and Mr. JULIUS HÜBNER, Lecturer in the Faculty of Technology in the University of Manchester, were elected ordinary members of the Society.

Ordinary Meeting, October 15th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Professor WILLIAM J. POPE, F.R.S., read a paper, written in conjunction with Mr. WILLIAM BARLOW, F.C.S., F.G.S., entitled **"The Relation between the Crystalline Form and the Chemical Constitution of Simple Inorganic Substances."**

The authors have applied the methods employed in their paper of October 16th, 1906, to the study of the crystalline structure and molecular condition of a number of simple inorganic substances such as the crystalline elements, binary compounds like silver iodide, potassium chloride, &c., ammonium halogen salts and compounds of the type of rubidium tri-iodide, RbI_8. In connection with the known, but hitherto unexplained fact, that the greater number of the crystallographically examined elements crystallise in the cubic system (50 per cent.) or the hexagonal system (35 per cent.), it is pointed out that only two simple homogeneous closest-packed assemblages of equal spheres exist, and that these possess holohedral cubic and hexagonal symmetry respectively. Further, the hexagonal closest-packed assemblage of equal spheres exhibits axial ratios of the form, $a : c = 1 : 0{\cdot}8165$, or $a : c = 1 : 1{\cdot}4142$, and the

hexagonal elements exhibit axial ratios approximating to one or other of these values. The fact that so large a proportion (85 per cent.) of the elements crystallise in either the cubic or the hexagonal system is therefore closely paralleled by the fact that their crystal structures can be graphically represented by homogeneous closest-packed assemblages of equal spheres in accordance with the authors' previous work.

It is shown that the crystalline forms affected by elements crystallising in systems of lower symmetry than the cubic and hexagonal can be derived by slight distortion from one or other of the two closest-packed assemblages above mentioned; the requisite distortion may result from molecular aggregation or from some other cause.

Most of the crystalline binary compounds consist of two elements of the same valency, and, in accordance with the authors' previous conclusions, atoms of the same valency must be represented in the crystalline edifice by spheres of approximately the same size. The reason is thus derived for the fact that most binary compounds crystallise in either the cubic (68·5 per cent.) or the hexagonal (19·5 per cent.) system. Homogeneous assemblages have, therefore, been constructed from spheres of two kinds, but of approximately equal size, which represent the crystalline structures of cubic and hexagonal compounds, such as AgI, KI, CaO, &c.; it is shown that the properties of these structures correspond very closely with those observed upon the crystalline materials. The dimorphism of silver iodide is elucidated by the simple manner in which the cubic closest-packed assemblage can be converted into that of hexagonal symmetry. A close agreement exists between the calculated axial ratios stated above for the hexagonal assemblage, and those observed upon the hexagonal binary compounds.

The crystalline structure appropriate to such substances as rubidium tri-iodide, RbI_3, thallic iodide, TlI_3, &c., is derived in a similar manner to the above, and it is shown that close agreement occurs between the derived and observed crystal forms. By the study of such crystalline substances it can be shown that

the spheres by which the alkali metals and the halogens are represented in the assemblages differ slightly in size, the sphere increasing in magnitude with an increase of atomic weight in the case of each of these classes of elements.

General Meeting, October 29th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Captain ARTHUR DOGGETT, Works Secretary, Vulcan Locomotive Works, Newton-le-Willows, *48, Gilda Brook Road, Eccles;* Mr. HAROLD SHAWCROSS LEIGH, *Brentwood, Worsley;* Mr. THEODORE GEORGE BENTLEY OSBORN, *Wellbury, Richmond Road, East Twickenham, Middlesex,* REGINALD FRANCIS GWYTHER, M.A., Secretary to the Joint Matriculation Board, and Mr. THOMAS WHITEHEAD, B.Sc., Chemist to the Manchester Steam Users' Association, were elected ordinary members of the Society.

Ordinary Meeting, October 29th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Dr. HENRY WILDE, F.R.S., read a paper entitled, "**On the Atomic Weight of Radium.**"

Professor E. RUTHERFORD, D.Sc., F.R.S., read a paper entitled, "**The Production and Origin of Radium.**" An account was given of the historical development of our ideas in regard to radium. On the disintegration theory, radium is regarded as a substance undergoing slow spontaneous transformation with a period of about 2,000 years. In order to account for the existence of radium in minerals of great age, it

is necessary to suppose that radium is produced from another substance of long period of transformation. There is an undoubted genetic connection between uranium and radium, for investigation has shown that the amount of radium in minerals is in all cases proportional to their content of uranium. If this be the case, radium should gradually appear in a preparation of uranium, initially freed from radium. No such growth of radium has been observed over a period of several years although a very minute growth of radium can be easily detected. This is not necessarily inconsistent with the disintegration theory for if one or more products of slow transformation exist between uranium and radium, no appreciable growth of the latter is to be expected in a short interval. A search for this intermediate product has recently proved successful. Boltwood found that a preparation of actinium, initially freed from radium, grew radium at a constant and rapid rate. Boltwood at first considered that actinium was this intermediate product and that actinium changed directly into radium. The growth of radium in actinium solutions was confirmed by the writer, who had commenced experiments in that direction three years before. The experiments showed, however, that actinium did not, as Boltwood supposed, change directly into radium. By a special method, a preparation of actinium was obtained by the writer which showed no appreciable growth of radium over a period of 240 days. The growth of radium, if it occurred at all, was certainly less than 1/500th of that ordinarily observed.

In another case, a solution of actinium was obtained which produced radium faster than the normal.

These results are completely explained by supposing that a new substance of slow transformation is present with actinium, and this substance is transformed directly into radium. This parent of radium has distinct chemical properties, which allow it to be separated from both actinium and radium. The absence of growth of radium observed in the actinium solution mentioned above is due to the fact that, by the special method, the parent of radium had been completely separated from the actinium.

In recent letters to *Nature*, Boltwood has confirmed the results of the writer, and has devised a satisfactory method of separating the radium parent from actinium. He has shown that this new body, which he proposes to call "ionium," gives out α and β rays, and has the chemical properties of thorium.

The Royal Society recently loaned the writer the actinium residues from about a ton of pitchblende. These residues contain the parent of radium, and experiments are in progress to isolate and concentrate both the actinium and ionium in these residues.

The President announced that Dr. Joseph Larmor, Secretary of the Royal Society and Lucasian Professor of Mathematics in the University of Cambridge, had consented to deliver the Wilde Lecture for 1908 on "The Physical Aspect of the Atomic Theory."

General Meeting, November 12th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Mr. JOHN WILLIAM BEWS, M.A., B.Sc., Lecturer in Economic Botany in the University of Manchester, was elected an ordinary member of the Society.

Ordinary Meeting, November 12th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent donations to the Society's Library:—"*De Berekening, de Bouw en het Bedrijf van het Kabelnet d. Gemeente Amsterdam,*" Door G. de Gelder (8vo., 's-Gravenhage, 1907), "*Mathematische und*

mikroskopisch-anatomische Studien über Blattstellungen nebst Betrachtungen über den Schalenbau der Miliolinen," von G. van Iterson, Jun. (4to, Jena, 1907), presented by the Technische Hoogeschool te Delft; "*Figures and descriptions of Canadian Organic Remains*," Decades 1, 3, and 4. (8vo., Montreal, 1858-59), "*The Fossil Plants of the Erian...and Upper Silurian Formations of Canada*," part 2, by J. W. Dawson (8vo, Montreal, 1882), "*Contributions to the Micro-palæontology of the Cambro-Silurian Rocks of Canada*," by A. H. Foord (8vo., Ottawa, 1882), "*A History of New Brunswick Geology*," by R. W. Ells (8vo, Montreal, 1887), "*Check-list of Canadian Plants*," by J. M. Macoun (8vo., Ottawa, 1889), "*Palæozoic Fossils*," vol. 3, pt. 3, by J. F. Whiteaves (8vo., Ottawa, 1897), "*Mesozoic Fossils*," vol. 1, pts. 4 & 5, by J. F. Whiteaves (8vo., Ottawa, 1900, 1903), "*Report on the Cambrian Rocks of Cape Breton*," by G. F. Matthew (8vo., Ottawa, 1903), "*Economic Minerals of Canada*," by G. M. Dawson (8vo., n. pl., 1900), "*On the Nepheline Rocks of Ice River, British Columbia*" (8vo., n. pl., [c. 1904]) and "*A Landslide on the Lievre River*" (8vo., [Ottawa, 1905]), by A. E. Barlow, presented by the Geological Survey of Canada; *Palæolithic Vessels of Egypt*, by R. de Rustafjaell (8vo., London, 1907), presented by the author; "*The History of the Collections in the Natural History Department of the British Museum*," vol. 2 (8vo., London, 1906), "*General Guide to the British Museum (Natural History)*," 11th ed. (8vo., London, 1906), "*Guide to the Fossil Reptiles, Amphibians and Fishes...in the British Museum (Natural History)*" (8vo., London, 1905), "*Guide to the Gallery of Reptilia and Amphibia in the...British Museum (Natural History)*" (8vo., London, 1906), "*Guide to the Galleries of Mammals (other than Ungulates) in the... British Museum (Natural History)*," 8th ed., (8vo, London, 1906), "*Guide to the great Game Animals (Ungulata), in the...British Museum (Natural History)*" (8vo, London, 1907), "*Guide to the Fossil Invertebrate Animals in the...British Museum (Natural History)*" (8vo., London, 1907), "*List of British Seed-plants and Ferns*"

(8vo, London, 1907), "*Guide to an Exhibition of Old Natural History Books*"...(8vo, London, 1905), "*Books and Portraits illustrating the History of Plant Classification*"...(8vo, London, 1906), "*Memorials of Linnæus*"...(8vo., London, 1907), presented by the Trustees of the British Museum.

Mr. D. M. S. WATSON, B.Sc., read two papers entitled respectively "**The Cone of** *Bothrodendron mundum* **(Will.),**" and "**On the Ulodendroid Scar.**"

The PRESIDENT announced that the Wilde Medal for 1908 would be awarded to Professor JOSEPH LARMOR, Sec.R.S., of Cambridge University, on March 3rd next, and that Professor Larmor would on that occasion deliver the Wilde Lecture on "The Physical Aspect of the Atomic Theory," and be afterwards entertained at a dinner in his honour, the particulars of which would be made known later.

General Meeting, November 26th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Mr. ABRAHAM FLATTERS, F.R.M.S., *16, Church Road, Longsight*, and Mr. ROBERT HENRY CLAYTON, B.Sc., Chemist, *Woodleigh, Blackfield Lane, Kersal, Manchester*, were elected ordinary members of the Society.

Ordinary Meeting, November 26th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Professor EDMUND KNECHT, Ph.D., gave a demonstration illustrating the formation of acetylene from elementary substances.

On heating a small piece of calcium on charcoal before the blowpipe, the metal readily took fire, and, after burning with a brilliant orange flame for about two seconds, sank into the mass of the charcoal. After the latter had been allowed to cool, it was broken up, when a hard lump was found which yielded acetylene on treatment with water.

Mr. JULIUS HÜBNER, M.Sc.Tech., F.I.C., read a paper entitled, **"New Reactions for the Characterisation of Mercerised Cotton."**

Mr. H. F. COWARD, M.Sc., read a paper, entitled **"The Direct Combination of Carbon and Hydrogen."** Doubt had recently been cast, he said, on the validity of Bone and Jerdan's synthesis of methane by direct union of carbon and hydrogen, by Berthelot, Pring and Hutton, and by Mayer and Altmayer.

The author, however, in recent experiments, made with small quantities of highly purified carbon, had obtained from 0·1 gram of carbon containing a maximum of 0·9 cc. of hydrogen, 100 to 120 cc. of methane by direct union with hydrogen.

Ordinary Meeting, December 10th, 1907.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

The PRESIDENT presented to the Society's Library copies of first editions of the following works of Hon. Robert Boyle, formerly in the possession of the Library of the old "Salford Academy":—

"Hydrostatical Paradoxes." 1666.

"New Experiments touching the Relation betwixt Flame and Air." London, 1672.

"Origine of Formes and Qualities." Oxford, 1667.

"Imperfection of the Chymist's Doctrine of Qualities." London, 1676.

The thanks of the members were voted to Professor Dixon for his interesting gift.

Mr. T. G. B. OSBORN stated that he had found on Saturday, December 7th, at Coal Clough, Burnley, a specimen of the fungus *Naematelia encephala* (Fries). The fact was of interest as this was a new locality for the species, no previous record for this district being known.

Mr. T. A. COWARD, F.Z.S., read a paper entitled "**Some Notes on the Mammals of Lundy Island.**"

Mr. C. GORDON HEWITT, M.Sc., read a paper entitled, "**Notes on some destructive Mites.**"

The above two papers are printed in full in the *Memoirs*.

General Meeting, January 14th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Mr. J. E. LITTLEWOOD, B.A., Richardson Lecturer in Mathematics in the Manchester University was elected an ordinary member of the Society.

Ordinary Meeting, January 14th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were among the recent accessions to the Society's Library:—"*Hints to Meteorological Observers in Tropical Africa*" (8vo., London, 1907), presented by the Meteorological Office of London; "*Rapporten van de*

Commissie in Nederlandsch-Indië voor Oudheid kundig Onderzoek ... 1905-06" (8vo., Batavia, 1907), presented by the Bataviaasch Genootschap van Kunsten en Wetenschappen; "*Flora Capensis*," vol. 4, sect. 1, pt. 4 (8vo., London, 1907), purchased; "*Hydrostatical Paradoxes*" (16mo., Oxford, 1666); "*The Origine of Formes and Qualities*" (12mo., Oxford, 1667); "*Tracts*" (16mo., London, 1672); "*The Mechanical Origine... of divers Particular Qualities*" (16mo., London, 1676), by Robert Boyle, presented by Professor H. B. Dixon, F.R.S.; "*Codex diplomaticus Lusatiae superioris iii.*"...Hft. 3, von R. Jecht (8vo., Görlitz, 1907), presented by the Oberlausitzische Gesellschaft der Wissenschaften; "*Seismogramme des nordpazifischen u. südamerik. Erdbebens, 16 Aug., 1906*" [with Diagrams], von E. Rudolph u. E. Tams (4to. and fol., Strassburg i. E., 1907); "*Katalog der im Jahre 1904 registrierten Seismischen Störungen*," von E. Rosenthal (4to., Strassburg i. E., 1907); "*Les Tremblements de Terre ressentis pendant l'année*, 1904," par E. Oddone (4to., Strassburg i. E., 1907), presented by Professor A. Schuster, F.R.S.

The PRESIDENT briefly referred to the death of Lord Kelvin, and mentioned that he had, in his capacity as President, represented the Society at the burial in Westminster Abbey, and at his suggestion it was resolved that the Society express, through their Council, to the family of the deceased man of science, their sense of the great loss sustained, through Lord Kelvin's death, by the world of science, and by this Society, of which he had been an Honorary Member since 1851.

Mr. T. A. COWARD, F.Z.S., exhibited a live specimen of the Greater Horse Shoe Bat (*Rhinolophus ferrum-equinum*) and briefly described its chief peculiarities.

Dr. E. C. EDGAR read a paper entitled "**The Atomic weight of Chlorine,**" which is printed in the *Memoirs*.

Mr. A. BROTHERS read a paper entitled "**On the Production of Photographs in the Colours of Nature,**"

of which the following is an abstract :—The development of colour photography dates back to the early part of the nineteenth century. The earliest methods were suggested by the discovery that a solar spectrum decomposes silver chloride, giving rise to a sympathetic colour change in the salt. (Seebeck 1810, Herschel, Zenker, Du Hauron, Lippman and many others). The next method consisted in using collodion plates (Abney and others) but no reliable means of fixing the pictures was known. The pictures obtained by the late Mr. Joseph Sidebotham—one of a red geranium with green leaves, and another of a landscape showing a red-tiled house and trees in shades of green—were accidental, and were never repeated, and it is curious that these results were fixed while others were quite fugitive. The introduction of orthochromatic plates made it possible to produce effects in colour not otherwise obtainable. As early as 1873, Dr. Vogel used gelatine bromide plates, and in 1879, Mr. F. E. Ives used collodio-bromide plates and obtained excellent results which could be used in the optical lantern. Three negatives were required, each taken through a differently coloured medium, the colours used being bright red, purple-blue and green. Transparencies were made which had the ordinary appearance except that they varied in density. To produce the colour effect three lanterns were used, the pictures were projected through the same coloured media, their images superposed, and the resulting picture approached very nearly the colours of nature. Ives also used an instrument called the Krōmscōp, which was stereoscopic and gave beautiful results. Mr. Thorp, too, devised an instrument which showed effects very similar to those of the Krōmscōp. Trichromatic printing or the "three-colour" process is the outcome of the work alluded to here. In this process, negatives are taken through colour-screens. The block from the negative of the red is printed in blue ink, that from the green in red, the one from the blue in yellow. The combination of the three pigments gives results which, in the ordinary way of colour printing, can only be obtained by using from 5 to 15 or 20 colours.

The nearest approach, it seems to me, to what has been the aim of so much research, may now be seen in the *Autochrome* Photography of Messrs. Lumière, of Lyons. Some details of the process were published by the authors some years since, but it is not possible to give full particulars here. The exact method of preparing the plate is not published, but it is known that the plates are coated with starch grains stained in three colours—violet, bright green, and bright orange. The glass has a *tacky* coating, and on this the starch grains are spread in an even layer, by dusting or otherwise. The spaces between the grains are filled with a black pigment.

This method is now said to be modified by crushing the granules until they fill up the spaces so as to cause the light to pass through the starch only. A coat of varnish protects the plate which is afterwards overlaid with an emulsion sensitive to the red rays. The plate is then ready for exposure in the camera. The photograph is taken through a yellow screen, the glass side of the plate being placed towards the lens.

The chief advantage of the new method is that, after the production of the plate, the ordinary routine of photographic work is all that is required to produce the transparency in colour.

General Meeting, January 28th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

Mr. THOMAS WILLIAM FOX, M.Sc.Tech., Professor of Textiles in the School of Technology, Manchester, *15, Clarendon Crescent, Eccles*, and Mr. WILLIAM MYERS, Lecturer in Textiles in the School of Technology, Manchester, *Stone Edge, Marple*, were elected ordinary members of the Society.

Ordinary Meeting, January 28th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

The PRESIDENT stated that on March 3rd, the occasion of the Wilde Lecture, the Lecturer, Dr. J. LARMOR, Sec.R.S., would be entertained at a dinner in his honour at the Midland Hotel, and the price of the tickets would be 7s. 6d., and not 10s. 6d. as formerly.

The attention of the members was directed to the publications of the International Seismological Association which were on the table and were presented to the Society by Professor A. Schuster, F.R.S. They consisted chiefly of a number of carefully prepared and important diagrams of earthquake movements which occurred in the North Pacific and South America in August, 1906.

The thanks of the meeting were voted to Professor Schuster for his interesting and valuable gift.

At this point the chair was occupied by Professor H. LAMB, LL.D., D.Sc., F.R.S.

Mr. A. STEPHENSON read a paper entitled "**On a New Type of Dynamical Stability.**"

The paper is printed in full in the *Memoirs*.

General Meeting, February 11th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President in the Chair.

Mr. H. BATEMAN, B.A., Reader in Mathematical Physics in the University of Manchester, was elected an ordinary member of the Society.

Ordinary Meeting, February 11th, 1908.

Professor H. B. DIXON, M.A., FRS., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. C. L. BARNES, M.A., and Mr. H. B. KNOWLES, M.A., were nominated Auditors of the Society's Accounts for the Session 1907-08.

Professor E. RUTHERFORD, D.Sc., F.R.S., read a paper written in conjunction with Dr. H. GEIGER, entitled:—"**A Method of Counting the Number of α-Particles from Radio-active Matter.**"

The paper is printed in the *Memoirs*.

Special Meeting, March 3rd, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The Wilde Lecture on "**The Physical Aspect of the Atomic Theory,**" was delivered by Professor JOSEPH LARMOR, D.Sc., Sec.R.S.

Afterwards the PRESIDENT presented to Professor LARMOR the Wilde Medal which had been awarded to him that session by the Council.

Ordinary Meeting, February 25th, 1908.

Professor H. B. DIXON, M.A., F.R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Dr. W. E. HOYLE, exhibited several well-preserved examples of fossil insects which were sent to the Manchester Museum from Shiobara, Japan, by Dr. Marie C. Stopes. Three were Diptera, one being probably a Calypterate, whilst another was a remarkably well-preserved mosquito probably of the family *Culicidae*, another appeared to be a Culicid larva, whilst the remaining specimen strongly resembled a *Machilis*. They were all derived from Tertiary deposits and were probably of Pleistocene age. No systematic work, however, seems to have been done as yet on these strata.

Mr. T. A. COWARD, F.Z.S., read a paper entitled "**Notes on the Greater Horseshoe Bat** (*Rhinolophus ferrum-equinum*) **in captivity**," which is printed in the *Memoirs*.

The Chair was taken at this point by Mr. FRANCIS NICHOLSON, F.Z.S.

Miss MARY MCNICOL, M.Sc., read a paper entitled "**On Cavity Parenchyma and Tyloses in Ferns**," of which the following is an abstract.

Cavity parenchyma, a tissue replacing to a greater or less degree, the protoxylem of the petiolar bundles, has been recorded in all the large groups of true Ferns, and also in the Water-fern *Marsilia*. In *Microlepia*, one of the Polypodiaceae, which represents a typical example, there is in the leafstalk a single curved bundle with slightly hooked ends: in each bundle there are generally five or six protoxylem groups, and of these one is found at each hook of the bundle, the other three or four lying in intermediate positions. The tissue is formed by the enlargement of the cells of the parenchymatous layer surrounding the xylem: these cells press in between the

spirally thickened portions of the vessel and by the enlargement of the tylose-like processes so formed, break up the vessels, replacing them by a strand of large-celled parenchyma. In *Nephrolepis* the passage of undivided xylem sheath cells through a series of two or three vessels can be seen. Other plants examined in which cavity parenchyma is present, were *Struthiopteris*, *Cheilanthes*, *Gymnogramme*, *Pteris*, *Marattia*, *Angiopteris*, *Osmunda*, *Gleichenia*, *Aneimia*, *Helminthostachys*, *Alsophila*, *Dicksonia*, *Hemitelia*, and *Cibotium*. In the last named occurred a condition not found in any other plant. Throughout the length of the strands of cavity parenchyma, some of the cells were lignified in a reticulate manner as tracheids resembling the "Speichertracheiden" described by Haberlandt. These lignified cells occurred sometimes singly sometimes in groups, but the groups were not connected with each other, and were thus probably not for conducting, but for storage purposes. The other cells of the strand were of the usual soft-celled type.

The occurrence of such tylose-like formations in recent ferns may be compared with the tyloses filling up the metaxylem tracheids in some of the fossil ferns, as in *Rhachiopteris insignis*, their presence in this case being due to the activity of all the cells of the xylem sheath and not simply of those bordering on the protoxylem.

General Meeting, March 10th, 1908.

DR. W. E. HOYLE, F.R.S.E., Vice-President, in the Chair.

Mr. W. H. FOWLER, M.Inst.C.E., was elected an ordinary member of the Society.

Ordinary Meeting, March 10th, 1908.

Dr. W. E. HOYLE, F.R.S.E., Vice-President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table. The following were among the recent accessions to the Society's Library:—"*Bibliographia Linnaeana*" ...part. 1, livr. 1, par J. M. Hulth (8vo., Uppsala, 1907), presented by the K. Vetenskaps Societeten i Upsala; "*Report on the geology...of...Northwest Quarter-St. No. 122*," by R. W. Ells (8vo., Ottawa, 1907); "*The Barytes deposit of Lake Ainslie and North Cheticamp*," by H. S. Poole (8vo., Ottawa, 1907); "*Report on the Cascade coal basin Alberta*" [with maps], by D. B. Dowling (8vo., Ottawa, 1907); "*Moose Mountain District of Southern Alberta*," by D. D. Cairnes (8vo., Ottawa, 1907); "*Report of the section of Chemistry and Mineralogy*" (8vo., Ottawa, 1906), presented by the Geological Survey of Canada; "*The Great Trigonometrical Survey of India*," vol. 18 (4to., Dehra Dun, 1906), presented by the Trigonometrical Survey of India; "*Skeletal Remains suggesting or attributed to early man in N. America*," by A. Hrdlicka (8vo., Washington, 1907), presented by the Bureau of American Ethnology; and "*A Monograph of the British Annelids*," vol. 2, pt. 1, by W. C. McIntosh (fol, London, 1908), purchased from the Ray Society.

MR. HENRY SIDEBOTTOM read a paper entitled—"**Report of the Recent Foraminifera from the Coast of the Island of Delos (Grecian Archipelago), Part V.**"

Many beautiful drawings of Foraminifera were exhibited, and some mounted specimens were shown under the microscope.

The paper is printed in full in the *Memoirs*.

MR. FRANCIS JONES, M.Sc., F.R.S.E., read a paper entitled "**The Action of Selenium and Tellurium on Arsine and Stibine**," which is printed in full in the *Memoirs*.

Ordinary Meeting, March 24th, 1908.

Professor H. B. DIXON, M.A., F R.S., President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. G. A. DUNLOP read a paper (communicated by Mr. T. A. Coward, F.Z.S.), entitled **"An Annotated List of the Alien Plants of the Warrington District."**

The paper will be published in the *Memoirs*.

Mr. T. A. COWARD, F.Z.S., read a paper written by Mr. Charles Oldham, F.Z.S., M.B.O.U., entitled **"Field Notes on the Birds of the Ravenglass Gullery, 1906."**

The author described in his paper the habits, during the breeding season, of the Black-headed Gull, Common, Lesser, and Sandwich Terns, as observed by him at Ravenglass, on the Cumberland Coast. The term "gullery" he applied to that portion of the sandhills which is occupied by colonies of these birds. He also mentioned other species—such as the Oyster-catcher and Sheld-duck—which nest in or in the immediate vicinity of the "gullery."

He referred to the change in the vegetation of the sandhills, caused by the presence of the unusual number of birds, and also drew special attention to a habit of the young Sandwich Tern, which obtains concealment from enemies by partially burying itself in the sand.

Ordinary Meeting, April 7th, 1908.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., Vice-President, in the Chair.

The thanks of the members were voted to the donors of the books upon the table.

Mr. R. L. TAYLOR, F.C.S., communicated a paper by Mr. ROBERT PETTIGREW, entitled **"On the Occurrence of Quartz Crystals in Limestone, Columnar Coal, Marble, &c."**

Photographs, microscopic and lantern slides were exhibited, showing beautiful microscopic crystals of quartz obtained from mountain limestone, columnar coal from Airdrie, in Lanarkshire, and ordinary statuary marble. Attention has been previously drawn to the occurrence of these crystals in limestone and marble,* but most of the members of the Society present had not had an opportunity of seeing them.

The crystals from limestone and marble are obtained by dissolving some quantity of the material in hydrochloric acid and treating the insoluble portion with strong sulphuric acid and a little potassium chlorate to destroy the organic matter, and then mounting in the usual way. The columnar coal, from which some of the specimens were obtained, resembles in some respects a well-burned coke. It comes from Airdrie, in Lanarkshire, and has been metamorphosed through an intrusive mass of basalt forcing its way along the seam of coal, with the result that the coal has been "coked" to a columnar mass. From this coal the crystals are obtained by grinding it coarsely and washing away the coal.

The crystals from both the limestone and the coal are mostly well formed double-ended quartz crystals, in many cases, especially in the smaller crystals, the facets being perfect, as if polished. They vary in size from about $\frac{1}{1000}$th to $\frac{1}{500}$th of an inch in length. When mounted in Canada Balsam (which has almost the same refractive index as quartz), a hazy outline only of the crystal is visible, but nearly every crystal shows a dark nucleus on which the crystal has been built. It is Mr. Pettigrew's opinion that this nucleus has been at one time Iron Pyrites, although it is now mostly oxidised to the brown oxide of iron.

The crystals in marble, owing to the pressure and meta-

* J. Barnes, *Trans. Manchester Geol. Soc.*, vol. 27, part 15.

said that, in a paper read before this Society in November last, on "New Reactions for the Characterisation of Mercerised Cotton," he pointed out that the action of other salts and acids in the presence of very small quantities of iodine was still under investigation. He had found in the meantime that characteristic reactions on mercerised cotton were produced by a large number of acids and salts in the presence of iodine. Among others he wished to mention here oxalic acid, citric acid, formic acid, and tartaric acid. With all these acids mercerised cotton exhibited a greater reactivity than did ordinary cotton. Glacial acetic acid was found to be less suited for this purpose.

Annual General Meeting, April 28th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The Annual Report of the Council and the Statement of Accounts were presented, and it was resolved:—"That the Annual Report, together with the Statement of Accounts, be adopted, and that they be printed in the Society's *Proceedings*."

Mr. C. GORDON HEWITT and Mr. H. B. KNOWLES were appointed Scutineers of the balloting papers.

The following members were elected officers of the Society and members of the Council for the ensuing year:—

President: H. B. DIXON, M.A., F.R.S.

Vice-Presidents: Sir WILLIAM H. BAILEY, M.I.Mech.E.; FRANCIS JONES, M.Sc., F.R.S.E.; W. E. HOYLE, M.A., D.Sc., F.R.S.E.; THOMAS THORP, F.R.A.S.

Secretaries: F. W. GAMBLE, D.Sc., F.R.S.; R. L. TAYLOR, F.C.S., F.I.C.

Treasurer: ARTHUR MCDOUGALL, B.Sc.

Librarian: C. L. BARNES, M.A.

Other Members of the Council: FRANCIS NICHOLSON, F.Z.S.; CHARLES BAILEY, M.Sc., F.L.S.; ERNEST F. LANGE, M.I.Mech.E., F.C.S.; W. J. POPE, F.R.S., F.C.S.; HORACE LAMB, M.A., LL.D., D.Sc., F.R.S.; ERNEST RUTHERFORD, F.R.S.

Ordinary Meeting, April 28th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Mr. THOMAS THORP, F.R.A.S., described a simple means for producing the characteristic glow in insulated vacuum tubes. If a vacuum tube be rubbed with the dry hand a very faint phosphorescent glow is observed in the dark, but if it be covered with a thin film of celluloid—say, by coating it with a solution of celluloid in acetone—the glow is much more easily induced. If instead of rubbing the vacuum tube itself, a glass plate coated with celluloid be rubbed and the vacuum tube brought near it from either side, the glow will be induced for several to and fro movements of the tube. Again, if the vacuum tube be inserted in a larger glass tube, the glow will be induced in like manner on rubbing the latter with the dry hand, but much more intensely if it has a celluloid coating. A still better effect is produced if the glass itself is rubbed with a sleeve of thin celluloid. Adopting the latter arrangement and securing a Neon vacuum tube in its interior, the characteristic glow is very pronounced and from ten to twenty distinct discharges are given for each stroke of the rubber, as may be plainly seen by giving the tube a sideway motion when rubbing. The Neon tube was suspended at about the centre of the outer tube by means of dry corks, and was not earthed in any way. The explanation appears to be, that in the disturbance set up by the friction, the electric field produced causes the transference of the free electrons in the walls of the exhausted tube from one position to another, and these by colliding with the gaseous molecules having relatively long free paths, are the source of the energy directly producing the glow.

Professor F. E. WEISS, D.Sc., F.L.S., exhibited some interesting botanical specimens brought by him from the Riviera.

One of these was the thickened base of a stem of the tree-heath, *Erica arborea*, the French "bruyère" from which the so-called briarwood pipes are made; the others were the fruits of an abnormal variety of *Citrus medica*, the citron, in which the carpels, instead of forming a continuous covering, are separated into finger-like processes which have given rise to the fanciful name of "Buddha's fingers" by which the fruits are known in certain localities.

Dr. GILBERT J. FOWLER, F.I.C., read a paper (communicated by Professor H. B. DIXON, M.A., F.R.S.) entitled "**Some Observations on the Chemical Effect of Tropical Sunlight.**"

General Meeting, May 12th, 1908.

Dr. W. E. HOYLE, F.R.S.E., Vice-President, in the Chair.

The Rt. Rev. J. E. C. WELLDON, D.D., Dean of Manchester, and Mr. CHARLES PRESTWICH SCOTT, M.A., *The Firs, Fallowfield*, were elected ordinary members of the Society.

Ordinary Meeting, May 12th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were among the recent accessions to the Society's Library:—"*Index du répertoire bibliographique des sciences mathématiques*," nouv. éd. (8vo., Amsterdam, 1908), presented by the Wiskundig Genootschap of Amsterdam;

"*The Structure of the Cotton Fibre*," by F. H. Bowman (12mo., London, 1908), presented by the author; "*Flora Capensis*," ed. by Sir W. T. Thiselton Dyer, vol. 4, sect. 1, part 5 (8vo., London, 1908), purchased; "*J. G. Böhm: Die Kunst-Uhren auf der...Sternwarte zu Prag*," von L. Weinek (4to., Prag, 1908), presented by the Sternwarte zu Prag; "*Idea dell' Universo... Note*," di G. C. Paoli (8vo., Milano, 1907-8), presented by the author; "*Astrographic Catalogue 1900·0. Oxford section Dec. +24° to +32°*"...[prepared] under the direction of H. H. Turner...vols. 3, 4 (4to., Edinburgh, 1907-8), presented by the University Observatory of Oxford; and "*Osservazioni meteorologiche e geodinamiche eseguite nell' anno 1906 nell' Osservatorio del Seminario Patriarcale di Venezia*" (4to., Venezia, 1907), presented by the Reale Istituto Veneto di Scienze, Lettere ed Arti.

Mr. T. G. B. OSBORN exhibited an unusual example of the furze or gorse, which he found at Hatchmere, in which the spines, instead of being erect, were recurved. As the plant was not then in flower the specific name could not be assigned with certainty. It was not *Ulex Europea*, but might be a new variety of *U. Gallii*.

Miss NELLIE SNAPE, B.Sc., read a paper, communicated by Professor F. E. WEISS, D.Sc., F.L.S., entitled "**Spore formation in the genus** *Chaetoceros*," of which the following is an abstract:—

Before the decrease in the number of individuals of *Chaetoceros* in the plankton during the late summer, there occurs the formation of the characteristic spined resting spores.

The protoplasmic contents of the organism withdraw from the old frustules and are surrounded by two new ones, one of which is in most cases provided with fine branching excrescences. The spores are set free by the breaking up of the *Chaetoceros* filament and the throwing off of the old shells. The whole process is apparently a fairly rapid one, and the result of some sudden change of conditions.

One of these was the thickened base of a stem of the tree-heath, *Erica arborea*, the French "bruyère" from which the so-called briarwood pipes are made; the others were the fruits of an abnormal variety of *Citrus medica*, the citron, in which the carpels, instead of forming a continuous covering, are separated into finger-like processes which have given rise to the fanciful name of "Buddha's fingers" by which the fruits are known in certain localities.

Dr. GILBERT J. FOWLER, F.I.C., read a paper (communicated by Professor H. B. DIXON, M.A., F.R.S.) entitled "**Some Observations on the Chemical Effect of Tropical Sunlight.**"

General Meeting, May 12th, 1908.

Dr. W. E. HOYLE, F.R.S.E., Vice-President, in the Chair.

The Rt. Rev. J. E. C. WELLDON, D.D., Dean of Manchester, and Mr. CHARLES PRESTWICH SCOTT, M.A., *The Firs, Fallowfield*, were elected ordinary members of the Society.

Ordinary Meeting, May 12th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were among the recent accessions to the Society's Library:—"*Index du répertoire bibliographique des sciences mathématiques*," nouv. éd. (8vo., Amsterdam, 1908), presented by the Wiskundig Genootschap of Amsterdam;

threads so as to preserve them in position when the epithelium fell away. This is an additional proof that the practice of mummifying was resorted to at a much earlier date than had been suggested by some Egyptologists, who had assigned 1600 B.C. as the time of its commencement. The practice was rapidly discontinued on the introduction into Egypt of Christianity.

Why mummification was instituted was not precisely known. It is held by some believers in re-incarnation that the soul cannot be re-incarnated whilst the body remains; it is quite possible that the Egyptians were averse to re-incarnation, and devised this means of preventing it.

On the coffin were a number of inscriptions, which were variants of what are known as the Pyramid Texts. Some of these read as follows:—"Thy mother Nut spreads herself above thee; she causes thee to be as a god without enemies"; "Comes to thee, comes to thee thy mother Nut;" "To Anubis, Lord of Sepa, may he grant that thou cross heaven, and that thou reach land at the pure places which are in heaven."

Dr. CAMERON pointed out the features of anatomical interest in the remains. The most noticeable character of the bones which lay upon the table was their slenderness. Measurements of the capacity and "indices" of the skull showed that it compared favourably with the average modern European type. The limb bones, on the other hand, rather tended towards the Simian character. Other markings indicated that Khnumu Nekht was of lethargic habit and much given to squatting. His height was probably between 5ft. 3in. and 5ft. 10in., and his age between sixty and seventy years. The teeth were of peculiar interest, as, with the exception of one that must have been absent before death, they were intact, but extraordinarily worn down, showing that the food eaten must have been exceedingly gritty.

Mr. JULIUS HÜBNER pointed out that flax was the chief material of the wrappings: there was no cotton or wool. One

of the fibres he had not then been able to identify, but it resembled "China grass."

Professor DIXON said he had examined some of the powder into which the body had decomposed, and found that it contained an organic salt of sodium, and that the solution in water gave a lather when shaken; it was in fact a kind of soap.

A number of beetle shells found in the coffin were exhibited, and Dr. Hoyle said that they were identical with those of a modern species, *Gibbium scotias*, which fed on hair and various fibres.

Annual Report of the Council, April, 1908.

The Society began the session with an ordinary membership of 150. During the present session 19 new members have joined the Society. Thirteen resignations have been received, and there has been one death, *viz.:* Mr. JAMES WHITEHEAD. This leaves on the roll 155 ordinary members. The Society has also lost, by death, 7 honorary members, *viz.:* Sir BENJAMIN BAKER, K.C.M.G., F.R.S.; Dr. ALEXANDER BUCHAN, F.R.S.; Lord KELVIN, O.M., G.C.V.O., F.R.S.; Sir W. H. PERKIN, F.R.S.; Dr. E. J. ROUTH, F.R.S.; Dr. HENRY C. SORBY, F.R.S.; and Professor C. A. YOUNG. Memorial notices of these gentlemen appear at the end of this report.

The average attendance at the meetings was 32 as compared with 24 for the session 1906-07.

The Society commenced the session with a balance in hand of £214. 16s. 7d., from all sources, this amount being made up of the following balances:—

At the credit of	General Fund.....	£26	16	0
„ „	Wilde Endowment Fund...	71	12	9
„ „	Joule Memorial Fund.....	80	6	2
„ „	Dalton Tomb Fund	36	1	8
		£214	16	7

The total balance in hand at the close of the session amounted to £208. 19s. 6d., and the amounts standing at the credit of the separate accounts, on the 31st March, 1908, are the following:—

At the credit of	General Fund.....	£17	0	9
„ „	Wilde Endowment Fund...	66	18	1
„ „	Joule Memorial Fund	88	1	0
„ „	Dalton Tomb Fund	36	19	8
Balance 31st March, 1908.....		£208	19	6

morphosis in the transformation to marble, are rarely perfect, but generally occur as cleavage plates, due to shearing. Many of the crystals appear to have split up, and look almost like cross sections, being hexagonal. Mr. Pettigrew has also found an insoluble residue when clear crystallised calcite or calc spar from Derbyshire is dissolved in hydrochloric acid. This residue consists of pyrites and of quartz, with moulds or hollows showing where the pyrites have been imbedded. The quartz crystals are cubic, and show in most cases exact counterparts of pyrites crystals, proving them to be pseudomorphs, after pyrites. This shows, of course, that the pyrites crystals were formed first, and that the silica is due to a secondary process of crystallisation by replacement.

Professor EDMUND KNECHT, Ph.D., M.Sc.Tech., F.I.C., read the following paper, entitled "**Note on the Action of Oxalic Acid on Cellulose.**"

Although oxalic acid has been used in calico printing for a very long time, it has not until comparatively recently been supposed to exert an influence upon cellulose different to that of any other acid of similar strength. In 1902, however, there appeared in *The Dyer and Calico Printer* an anonymous article (which I have since ascertained was written by one of my former students, Mr. H. H. Pilkington), in which it is pointed out that if calico is printed with thickened oxalic acid, and is then allowed to dry in a cool place for twelve hours, and subsequently washed, the printed parts show an increased affinity for basic colours, and a decreased affinity for so-called direct colours. The cloth is not tendered in the printed places. Citric and tartaric acids do not produce this effect. The author merely recorded these interesting facts, but offered no explanation.

In view of these interesting results it appeared to me desirable to attempt to find some explanation of this remarkable behaviour of oxalic acid, and I consequently first repeated the experiments, and was able to verify the results.

I found that by boiling the treated fabric first in alcohol, and

then in ether, the effect could not be removed. By boiling, however, in a dilute solution of caustic soda for a few minutes, all trace of the action of the oxalic acid vanished, without any tendering of the fabric being noticeable. This observation precluded, therefore, the possibility of the formation of oxycellulose. It almost appeared as if some of the oxalic acid had combined with the cellulose to form an oxalate, which, by analogy with the nitrates, acetates, &c., would show the characteristic behaviour towards dyestuffs. I was unable, however, to detect oxalic acid in the caustic soda extract of the treated fabric, and for some time gave up the work.

On resuming my investigation into the subject, I was led to test the caustic soda extract of the treated fabric for formic acid, which was found to be present. It would therefore appear that in drying the oxalic acid had undergone decomposition into carbonic acid and formic acid, which latter had in the nascent state combined with the cellulose. Attempts to determine quantitatively the actual amounts of cellulose formiate produced were not satisfactory, the results obtained being very irregular. In any case, however, the amounts found appear to be only small.

This explanation of the action of oxalic acid on cellulose becomes all the more probable from the results of further experiments with malonic acid and hexylmalonic acid, both of which behave like oxalic acid, and this would be expected from their behaviour on heating. In the saponification product from cotton treated with malonic acid, it was possible to detect acetic acid.

Succinic acid, on the other hand, does not show any alteration in the cellulose, nor does glutaric acid.

It would appear, therefore, that the action of oxalic acid on cellulose simply constitutes one example of a general mode of formation of acidyl celluloses.

Professor KNECHT also exhibited specimens of commercial metallic titanium and commercial silicon.

In a discussion which took place later Mr. JULIUS HÜBNER

said that, in a paper read before this Society in November last, on "New Reactions for the Characterisation of Mercerised Cotton," he pointed out that the action of other salts and acids in the presence of very small quantities of iodine was still under investigation. He had found in the meantime that characteristic reactions on mercerised cotton were produced by a large number of acids and salts in the presence of iodine. Among others he wished to mention here oxalic acid, citric acid, formic acid, and tartaric acid. With all these acids mercerised cotton exhibited a greater reactivity than did ordinary cotton. Glacial acetic acid was found to be less suited for this purpose.

Annual General Meeting, April 28th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The Annual Report of the Council and the Statement of Accounts were presented, and it was resolved:—"That the Annual Report, together with the Statement of Accounts, be adopted, and that they be printed in the Society's *Proceedings*."

Mr. C. GORDON HEWITT and Mr. H. B. KNOWLES were appointed Scutineers of the balloting papers.

The following members were elected officers of the Society and members of the Council for the ensuing year:—

President: H. B. DIXON, M.A., F.R.S.

Vice-Presidents: Sir WILLIAM H. BAILEY, M.I.Mech.E.; FRANCIS JONES, M.Sc., F.R.S.E.; W. E. HOYLE, M.A., D.Sc., F.R.S.E.; THOMAS THORP, F.R.A.S.

Secretaries: F. W. GAMBLE, D.Sc., F.R.S.; R. L. TAYLOR, F.C.S., F.I.C.

Treasurer: ARTHUR MCDOUGALL, B.Sc.

Librarian: C. L. BARNES, M.A.

Other Members of the Council: FRANCIS NICHOLSON, F.Z.S.; CHARLES BAILEY, M.Sc., F.L.S.; ERNEST F. LANGE, M.I.Mech.E., F.C.S.; W. J. POPE, F.R.S., F.C.S.; HORACE LAMB, M.A., LL.D., D.Sc., F.R.S.; ERNEST RUTHERFORD, F.R.S.

Ordinary Meeting, April 28th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables.

Mr. THOMAS THORP, F.R.A.S., described a simple means for producing the characteristic glow in insulated vacuum tubes. If a vacuum tube be rubbed with the dry hand a very faint phosphorescent glow is observed in the dark, but if it be covered with a thin film of celluloid—say, by coating it with a solution of celluloid in acetone—the glow is much more easily induced. If instead of rubbing the vacuum tube itself, a glass plate coated with celluloid be rubbed and the vacuum tube brought near it from either side, the glow will be induced for several to and fro movements of the tube. Again, if the vacuum tube be inserted in a larger glass tube, the glow will be induced in like manner on rubbing the latter with the dry hand, but much more intensely if it has a celluloid coating. A still better effect is produced if the glass itself is rubbed with a sleeve of thin celluloid. Adopting the latter arrangement and securing a Neon vacuum tube in its interior, the characteristic glow is very pronounced and from ten to twenty distinct discharges are given for each stroke of the rubber, as may be plainly seen by giving the tube a sideway motion when rubbing. The Neon tube was suspended at about the centre of the outer tube by means of dry corks, and was not earthed in any way. The explanation appears to be, that in the disturbance set up by the friction, the electric field produced causes the transference of the free electrons in the walls of the exhausted tube from one position to another, and these by colliding with the gaseous molecules having relatively long free paths, are the source of the energy directly producing the glow.

Professor F. E. WEISS, D.Sc., F.L.S., exhibited some interesting botanical specimens brought by him from the Riviera.

One of these was the thickened base of a stem of the tree-heath, *Erica arborea*, the French "bruyère" from which the so-called briarwood pipes are made; the others were the fruits of an abnormal variety of *Citrus medica*, the citron, in which the carpels, instead of forming a continuous covering, are separated into finger-like processes which have given rise to the fanciful name of "Buddha's fingers" by which the fruits are known in certain localities.

Dr. GILBERT J. FOWLER, F.I.C., read a paper (communicated by Professor H. B. DIXON, M.A., F.R.S.) entitled "**Some Observations on the Chemical Effect of Tropical Sunlight.**"

General Meeting, May 12th, 1908.

Dr. W. E. HOYLE, F.R.S.E., Vice-President, in the Chair.

The Rt. Rev. J. E. C. WELLDON, D.D., Dean of Manchester, and Mr. CHARLES PRESTWICH SCOTT, M.A., *The Firs, Fallowfield*, were elected ordinary members of the Society.

Ordinary Meeting, May 12th, 1908.

The President, Professor H. B. DIXON, M.A., F.R.S., in the Chair.

The thanks of the members were voted to the donors of the books upon the tables. The following were among the recent accessions to the Society's Library:—"*Index du répertoire bibliographique des sciences mathématiques*," nouv. éd. (8vo., Amsterdam, 1908), presented by the Wiskundig Genootschap of Amsterdam;

"*The Structure of the Cotton Fibre*," by F. H. Bowman (12mo., London, 1908), presented by the author; "*Flora Capensis*," ed. by Sir W. T. Thiselton Dyer, vol. 4, sect. 1, part 5 (8vo., London, 1908), purchased; "*J. G. Böhm: Die Kunst-Uhren auf der...Sternwarte zu Prag*," von L. Weinek (4to., Prag, 1908), presented by the Sternwarte zu Prag; "*Idea dell' Universo... Note*," di G. C. Paoli (8vo., Milano, 1907-8), presented by the author; "*Astrographic Catalogue 1900·0. Oxford section Dec. +24° to +32°*"...[prepared] under the direction of H. H. Turner...vols. 3, 4 (4to., Edinburgh, 1907-8), presented by the University Observatory of Oxford; and "*Osservazioni meteorologiche e geodinamiche eseguite nell' anno 1906 nell' Osservatorio del Seminario Patriarcale di Venezia*" (4to., Venezia, 1907), presented by the Reale Istituto Veneto di Scienze, Lettere ed Arti.

Mr. T. G. B. OSBORN exhibited an unusual example of the furze or gorse, which he found at Hatchmere, in which the spines, instead of being erect, were recurved. As the plant was not then in flower the specific name could not be assigned with certainty. It was not *Ulex Europea*, but might be a new variety of *U. Gallii*.

Miss NELLIE SNAPE, B.Sc., read a paper, communicated by Professor F. E. WEISS, D.Sc., F.L.S., entitled "**Spore formation in the genus** *Chaetoceros*," of which the following is an abstract:—

Before the decrease in the number of individuals of *Chaetoceros* in the plankton during the late summer, there occurs the formation of the characteristic spined resting spores.

The protoplasmic contents of the organism withdraw from the old frustules and are surrounded by two new ones, one of which is in most cases provided with fine branching excrescences. The spores are set free by the breaking up of the *Chaetoceros* filament and the throwing off of the old shells. The whole process is apparently a fairly rapid one, and the result of some sudden change of conditions.

Individuals of *Chaetoceros* are also often seen containing a number of small rounded spore-like bodies, formed by the contraction of the protoplasm and its aggregation round the chromatophores. The number of these bodies varies considerably, but their size is remarkably constant, notwithstanding the variation in the height of the filaments in which they are formed.

It would appear rather probable that these spore-like bodies are really gametes, but confirmatory evidence on this point is at present lacking.

Prof. F. E. WEISS, D.Sc., F.R.S., described specimens of a new type of Stigmaria, in which the medullary area contained numerous small spiral tracheids, very much like the first formed elements of the wood (protoxylem). These vessels were, however, disconnected from the primary wood and interspersed with parenchymatous cells. They were narrower and longer than the tracheids met with in the centre of the stem of *Lepidodendron Selaginoides*, and seemed from their structure to have had a conducting function.

In other respects the Stigmaria showed a typical structure. It had the usual appendages or rootlets, and the secondary wood seemed broken up by wide medullary rays. The woody cylinder is comparatively slight and the mid-cortex hardly preserved at all. The outer cortex is at a considerable distance from the central axis.

Miss MARGARET A. MURRAY, F.S.A., Scot., read a paper, communicated by Dr. W. E. HOYLE, F.R.S.E., entitled, **"On the Mummy of Khnumu Nekht in the Manchester Museum."** The coffin and its contents were found in a rock tomb at Rifeh, near Assiout, in Upper Egypt, and date from the 12th Dynasty, or, roughly, about 2,500 B.C. The name Khnumu Nekht, the writer said, meant the "Power of the Creator."

That this was a real mummy was shown by the fact that the nails of the hands and feet had been carefully bound with

threads so as to preserve them in position when the epithelium fell away. This is an additional proof that the practice of mummifying was resorted to at a much earlier date than had been suggested by some Egyptologists, who had assigned 1600 B.C. as the time of its commencement. The practice was rapidly discontinued on the introduction into Egypt of Christianity.

Why mummification was instituted was not precisely known. It is held by some believers in re-incarnation that the soul cannot be re-incarnated whilst the body remains; it is quite possible that the Egyptians were averse to re-incarnation, and devised this means of preventing it.

On the coffin were a number of inscriptions, which were variants of what are known as the Pyramid Texts. Some of these read as follows:—"Thy mother Nut spreads herself above thee; she causes thee to be as a god without enemies"; "Comes to thee, comes to thee thy mother Nut;" "To Anubis, Lord of Sepa, may he grant that thou cross heaven, and that thou reach land at the pure places which are in heaven."

Dr. CAMERON pointed out the features of anatomical interest in the remains. The most noticeable character of the bones which lay upon the table was their slenderness. Measurements of the capacity and "indices" of the skull showed that it compared favourably with the average modern European type. The limb bones, on the other hand, rather tended towards the Simian character. Other markings indicated that Khnumu Nekht was of lethargic habit and much given to squatting. His height was probably between 5ft. 3in. and 5ft. 10in., and his age between sixty and seventy years. The teeth were of peculiar interest, as, with the exception of one that must have been absent before death, they were intact, but extraordinarily worn down, showing that the food eaten must have been exceedingly gritty.

Mr. JULIUS HÜBNER pointed out that flax was the chief material of the wrappings: there was no cotton or wool. One

of the fibres he had not then been able to identify, but it resembled "China grass."

Professor DIXON said he had examined some of the powder into which the body had decomposed, and found that it contained an organic salt of sodium, and that the solution in water gave a lather when shaken; it was in fact a kind of soap.

A number of beetle shells found in the coffin were exhibited, and Dr. Hoyle said that they were identical with those of a modern species, *Gibbium scotias*, which fed on hair and various fibres.

Annual Report of the Council, April, 1908.

The Society began the session with an ordinary membership of 150. During the present session 19 new members have joined the Society. Thirteen resignations have been received, and there has been one death, *viz.:* Mr. JAMES WHITEHEAD. This leaves on the roll 155 ordinary members. The Society has also lost, by death, 7 honorary members, *viz.:* Sir BENJAMIN BAKER, K.C.M.G., F.R.S.; Dr. ALEXANDER BUCHAN, F.R.S.; Lord KELVIN, O.M., G.C.V.O., F.R.S.; Sir W. H. PERKIN, F.R.S.; Dr. E. J. ROUTH, F.R.S.; Dr. HENRY C. SORBY, F.R.S.; and Professor C. A. YOUNG. Memorial notices of these gentlemen appear at the end of this report.

The average attendance at the meetings was 32 as compared with 24 for the session 1906-07.

The Society commenced the session with a balance in hand of £214. 16s. 7d., from all sources, this amount being made up of the following balances:—

At the credit of	General Fund.....	£26	16	0
„ „	Wilde Endowment Fund...	71	12	9
„ „	Joule Memorial Fund.....	80	6	2
„ „	Dalton Tomb Fund	36	1	8
		£214	16	7

The total balance in hand at the close of the session amounted to £208. 19s. 6d., and the amounts standing at the credit of the separate accounts, on the 31st March, 1908, are the following:—

At the credit of	General Fund.....	£17	0	9
„ „	Wilde Endowment Fund...	66	18	1
„ „	Joule Memorial Fund	88	1	0
„ „	Dalton Tomb Fund	36	19	8
Balance 31st March, 1908.....		£208	19	6

The Wilde Endowment Fund, which is kept as a separate banking account shows a balance of £66. 18s. 1d., in its favour, as against £71. 12s. 9d., at the beginning of the financial year, the receipts from the invested funds being the same as last year.

During the session improvements in the lighting and ventilating of the Library were carried out, and the floor of the caretakers' room was covered with linoleum. The cost, amounting to £26. 2s. 5d., has been charged to the Wilde Endowment Fund.

The Librarian reports that during the session 897 volumes have been stamped, catalogued and pressmarked, 804 of these being serials, and 93 separate works. There have been written 415 catalogue cards, 292 for serials, and 123 for separate works. The total number of volumes catalogued to date is 30,673 for which 10,859 cards have been written.

Satisfactory use is made of the library for reference purposes, but the number of volumes consulted is not recorded. During the session, 205 volumes have been borrowed from the library, as compared with 176 in the previous session.

Some attention has continued to be paid to the completion of sets, 42 volumes or parts having been obtained, which complete three sets, and partly complete three. Of these, 8 volumes were purchased, and the rest were presented by the societies publishing them.

A larger amount of binding than usual has been done this session, 200 volumes having been bound in 183.

A record of the accessions to the library shows that, from April, 1907, to March, 1908, 812 serials and 110 separate works were received, a total of 922 volumes. The donations during the session (exclusive of the usual exchanges) amount to 106 volumes and 89 dissertations; 8 volumes have been purchased (in addition to the periodicals on the regular subscription list).

During the past session the Society has arranged to exchange publications with the following:—The University of New Mexico, Albuquerque; the Naturwissenshaftlicher Verein in Hamburg, complete sets of whose *Verhandlungen* and *Abhandlungen* have been acquired; and the Agricultural Research Institute, Pusa, Bengal.

The *Chemical Abstracts*, published by the American Chemical Society, have been added to the list of periodicals subscribed for.

The publication of the *Memoirs and Proceedings* has been continued under the supervision of the Editorial Committee.

The Society is indebted to the following gentlemen for the undermentioned gifts:—

Dr. F. W. Jordan, for engraved portraits of John Dalton, and of Thomas Young formerly Secretary of the Royal Society. The latter is from a painting by Sir Thomas Lawrence.

Professor H. B. Dixon, F.R.S., for copies of the following works by Robert Boyle:—"Hydrostatical Paradoxes," 1666; "New Experiments touching the Relation betwixt Flame and Air," 1672; "Origine of Formes and Qualities," 1667; and "Imperfection of the Chymist's Doctrine of Qualities," 1676.

Professor A. Schuster, F.R.S., for a set of the Publications of the International Seismological Association.

Mr. F. Nicholson, for defraying the cost of repairing and re-gilding the portrait of the Rev. George Walker, which is hung in the Council Room.

The Council awarded the Wilde Medal for 1908 to Dr. Joseph Larmor, Sec.R.S., Lucasian Professor of Mathematics in the University of Cambridge, for his researches on the constitution of matter and ether.

Professor Larmor was also invited to deliver the Wilde Lecture for 1908. The Lecture on "The Physical Aspect of the Atomic Theory" was delivered and the Medal was presented on Tuesday, March 3rd, 1908.

In response to a request made by the Smithsonian Institution of Washington, the Council resolved "that a set of the Society's publications, as complete as possible, be sent to the Californian Academy of Sciences to take the place of the set destroyed by the conflagration of San Francisco in April, 1906."

The Council resolved that the following document, appreciative of the work of Carl von Linné, be sent to the New York Academy of Sciences, to be read on May 23rd, 1907, the occasion of the celebration of the 200th anniversary of his birth.

"The Manchester Literary and Philosophical Society "willingly joins with the New York Academy of Sciences, in "its commemoration of the two hundredth anniversary of the "birth of the illustrious Linnæus.

"His profound insight into the affinities and disresem- "blances of organised beings; his vivid differentiation of "natural groups; his pithy crisp characterization of orders, "genera, and species, and his binomial principle of "nomenclature, all exercised a profoundly stimulating "influence upon the progress of biological science.

"Nor must the personal merits of the man pass "unrecognised; his acknowledgment of the work of his "predecessors; his self-sacrificing labours; the enthusiasm "with which he inspired his students; and his remarkable "humility—so fittingly commemorated in the *Linnæa* "*borealis*—are qualities which provoke the admiration of "naturalists, alike in the hemisphere in which he worked,

"and in the hemisphere in which this commemoration is "being held."

"Signed on behalf of the Manchester Literary and Philosophical Society (England),

HAROLD B. DIXON, *President.*

FRANCIS JONES,
FREDERICK WILLIAM GAMBLE, } *Hon. Secretaries.*

"*May 14th, 1907.*"

In response to a request from the Committee of the Science Section of the Franco-British Exhibition asking for the loan of apparatus and results of research work, the Council resolved "that a reply be sent saying that the Council are prepared to lend certain personal relics and apparatus used by Dalton in his teaching, but not the apparatus used by Dalton or Joule in their scientific researches."

SIR BENJAMIN BAKER, K.C.B., K.C.M.G., D.Sc., LL.D., F.R.S., died suddenly from heart failure in his sixty-seventh year on the 19th May, 1907, very soon after his return from Egypt, where he had just advised the heightening of the Assuan dam. Although his professional activity did not commence until after the middle of the last century, his name will in the future probably be coupled with those of the pioneers of modern engineering, for like them he opened up new ground, like them, too, his training was entirely a practical one. Although he was a most reliable authority on all scientific subjects connected with engineering, this was due to his natural inclination to examine each subject as it presented itself, by studying such scientific enquiries as were available and, where these failed, by making his own experiments. His last act typically illustrated his character, for when recently certain mathematicians asserted that his great work, the Assuan dam, was not strong enough to have its height increased, he re-examined the whole question from their point of

view, and, coming to the conclusion that their assertion was not correct, he had the courage in his old age to rely on his own deductions, and unhesitatingly recommended that the dam should be heightened as had been originally intended. This natural courage to take responsibility must have revealed itself at an early date, for the late Sir John Fowler made him, when only 29 years old, Chief Assistant Engineer on the construction of the District Railway, the first of its kind in London, and having innumerable and unforeseen difficulties. He had at the age of 22 joined Sir John Fowler's staff, his previous training from his 16th year being an apprenticeship, during which he was engaged on the Grosvenor Bridge, but he had even then engaged in scientific studies, and by papers on beams, bridge construction, etc., had established a reputation as an authority on the practice and theory of engineering. His great scientific and practical knowledge, combined with the reliability of his advice, caused him to be looked up to by the whole engineering profession, and led to his being associated with many of the largest and most novel undertakings both in England and abroad. He was consulted about the first London tube, the Central London tube, the Hudson River tunnel, railways all over the world, the Avonmouth Docks, the Hull Joint Docks, and many other undertakings, and rendered invaluable service on the Ordnance Committee on which he sat during the time that black powder was being replaced by cordite, and gun barrels of old construction by wire wound ones. The two great works with which his name will be lastingly associated are the Forth Bridge and the Nile dam. It would seem that several designs for the former were discussed by the Railway Companies, but when the Tay Bridge was blown down it was felt that, for a structure of the magnitude and novelty of the proposed Forth Bridge, Sir Benjamin Baker's views should carry the greatest weight. The bridge had to be its own scaffolding, and Sir Benjamin boldly adopted for this purpose the Cantilever design, though it was practically a novelty, and he carried it out on the grandest scale. It is not generally realised

that the spans of this bridge are nearly twice as great as the Eiffel tower is high, viz., 1,710 ft. against 984 ft. His other monumental work, the Assuan Dam, is not so original in design as the previous mentioned one, but here, too, material had to be dispensed with wherever possible in order not to overburden the structure. Some men, with less theoretical and practical knowledge than Sir Benjamin Baker had, might not have seen all the possible dangers and might have hoped to guard themselves against risks by piling on material; others, again, although quite aware of the seriousness of the problem, might have feared to recommend what had never been done before. Sir Benjamin's forte was that he clearly saw the nature of the problem in hand and had the courage to face the risks.

He was repeatedly honoured both at home and abroad; he became a Fellow of the Royal Society in 1890, received several decorations and honorary degrees, and was elected an Honorary Member of this Society in 1886. He was intimately associated with the Royal Institution, the British Association, and the Institutions of Mechanical Engineers and of Civil Engineers. Of the latter Institution he was President in 1895.

ALEXANDER BUCHAN was born at Kinnesswood, Kinross, on April 11th, 1829, and received his early training in the Free Church Normal School, Edinburgh. He proceeded to the University, and subsequently graduated as M.A. He decided to follow a scholastic career, and held office as a teacher at Banchory, Blackford, and Dunkeld. Owing, however, to a constitutional weakness of the throat, he abandoned this profession, and in 1860 accepted the office of Secretary of the Scottish Meteorological Society. Henceforth his energies were devoted to the furtherance of his favourite science with a consistent enthusiasm which won the respect and admiration of all his friends and colleagues. His clearness as an expositor is shown in his "Handy Book of Meteorology," which was published in

1867, a second edition being called for in the following year. It speedily went out of print, and it was always hoped that he would find leisure to re-issue it, with an account of recent researches; but this was not to be.

As an investigator he was remarkable for an amazing power of dealing with large masses of figures, which amounted almost to genius. He was an advocate of graphic methods, and was accustomed to set down his facts on maps and charts. By this means he was enabled to identify closed isobars as cyclonic and anticyclonic areas, and he was the first to trace the course of a "depression" across the Atlantic. Among his more important memoirs must be mentioned a paper read before the Royal Society of Edinburgh, entitled, "The Mean Pressure of the Atmosphere and the Prevailing Winds over the Globe for the Months and for the Year," (*Trans. Roy. Soc. Edin.*, vol. 25, 1869), which at once established his position as a leader among the meteorologists of the world, and led to his being entrusted with the preparation of a report on the enormous mass of data collected by H.M.S. "Challenger" during her memorable cruise round the world. This was published in 1889, among the reports of that expedition; it occupies 341 pages, and contains 52 maps showing the mean temperature, the mean barometric pressure and wind directions for each month, as well as for the whole year. This work was followed by the preparation (with the assistance of Dr. A. J. Herbertson) of the meteorological section of Bartholomew's great "Physical Atlas."

No account of Dr. Buchan's work would be complete without a reference to his connection with the Ben Nevis Observatory. He was one of its most enthusiastic promoters when it was founded in 1883, and for the remainder of his life the work of that station, on the highest point of the British Isles, unquestionably held the first place in his scientific interests. Some of the results have been published by the Royal Society of Edinburgh, but these were only introductory, and it was always his hope to extend and complete them.

In person, Dr. Buchan was tall and slight, with a flowing beard and pleasant genial features, which well expressed the character of the man. Among his friends he was commonly called "the Clerk of the Weather," and his almost invariable habit of wearing a long overcoat and carrying an umbrella led to the remark that he must have a profound distrust of the elements to whose study his life was devoted.

The esteem in which he was held by the scientific world is shown by the honours conferred upon him. He was made an Honorary LL.D. of Glasgow in 1887; from the Royal Meteorological Society he received the first award of the Symonds Memorial Gold Medal, and from the Royal Society of Edinburgh the Makdougall Brisbane, and Gunning prizes. He was elected a Fellow of the Royal Society in 1898, and an Honorary Member of this Society in 1886.

He passed away on May 13th, 1907, after a brief illness, leaving to science a mass of brilliant and useful work, to his pupils and colleagues an example of single-hearted devotion, and to his friends the memory of a gifted and lovable personality.

W. E. H.

Lord Kelvin.—In the following lines no attempt is made either to summarise Lord Kelvin's work, or give any estimate of his place among the leaders of scientific thought. But in one branch of research, Lord Kelvin has had a deeply interesting connexion with this Society: for Lord Kelvin's early recognition of the labours of Joule, and the intimate friendship and collaboration which sprang from that recognition, are part of the history of Thermodynamics. I shall only touch on this side of his work.

Joule had begun his great work in 1838 by constructing an electro-magnetic engine after Sturgeon's design, and determining the weight it would raise through one foot in a minute. In 1839, he had added a galvanometer of his own device and construction, and discovered the law connecting the attraction

of an electro-magnet with the strength of the current and the length of the wire; in 1840 he employed a Voltameter and measured the zinc used in supplying the current by means of the hydrogen liberated. His experiments led him to the remarkable generalisation, published in his first paper to this Society, that the heat of combustion of zinc was identical with the heat developed in the circuit of a zinc battery, and that the heat of combustion is the consequence of resistance to electrical conduction between Oxygen and the combustible. In 1843 came his classical experiment of measuring the heat developed in the coil of an electro-magnet made to rotate between the poles of a magnet by mechanical power or by a battery, in which he proved not only the equivalency between the heat developed and the chemical and electrical energies spent in driving an electro-magnetic engine, but also the equivalency between the heat developed and the mechanical effort in driving a magneto-electric machine.

Joule's first determination of the mechanical equivalent of heat was given to the British Association at Cork, in 1843. Immediately after the meeting he began his experiments on the friction of water, and then proceeded to work at the changes of temperature produced by the rarefaction and condensation of the air, which led him to the conception of the "absolute zero of temperature." But the leading physicists of this date were not prepared for the revolution of ideas inaugurated by Joule. His papers were rejected by the Royal Society; and for nearly six years no one in authority accepted, or even alluded to, Joule's views. The reasons for this silence were probably two-fold: the apparent contradiction between Joule's results and Carnot's theorem, and the small differences of temperature on which Joule relied.

In June, 1847, at the Oxford meeting of the British Association came the memorable meeting of Kelvin and Joule, when the latter read his paper before the Chemical Section, and was requested by the President "to cut it short." "This I

endeavoured to do," wrote Joule, "and a discussion not being invited, the communication would have passed without comment if a young man had not risen in the Section, and by his intelligent observations created a lively interest in the new theory. The young man was William Thomson."

Lord Kelvin has given his account of the meeting. "I heard his paper read at the Section, and felt strongly impelled to rise and say that it must be wrong, because the true mechanical value of heat given must, for small differences of temperature, be proportional to the square of its quantity. I knew from Carnot's law that this must be true. But as I listened on and on, I saw that though Carnot had vitally important truth not to be abandoned, Joule had certainly a great truth and a great discovery . . . I said my say to Joule at the end of the meeting. After that I had a long talk over the whole matter at one of the conversaziones, and we became friends from thence forward. . . . About a fortnight later I was walking down from Chamounix, and whom should I meet walking up but Joule, with a long thermometer in his hand, and a carriage with a lady in it not far off. He told me he had been married since we parted at Oxford, and he was going to try for elevation of temperature in waterfalls."

In the following year Kelvin alludes in a footnote to Joule's researches, but in 1849 his paper on Carnot's theory of the Motive power of Heat contains a more definite recognition: "The extremely important discoveries recently made by Mr. Joule, of Manchester, that heat is evolved in every part of a closed electric conductor moving in the neighbourhood of a magnet; and that heat is *generated* by the friction of fluids in motion, seem to overturn the opinion generally held that heat cannot be generated, but only produced from a source where it has previously existed." Kelvin could not then reconcile Joule's results with Carnot's theorem, which he considered must be still accepted as most probable. But in 1851 Kelvin read his classical memoir, showing that a complete theory of

point of view, and he strenuously opposed the recent changes which, as he thought, would tend to circumscribe and limit its influence in Cambridge education. His conservatism had, at any rate, this justification, that he could point to a long array of pupils who subsequently attained to the highest rank in science, at the bar, and in other professions. It is a little difficult to say wherein the secret of his success lay. Some subjects, such as dynamics, or analytical solid geometry, he taught with spirit; but he had little leaning towards philosophical questions, or towards advanced pure mathematics of any kind. Probably the simple explanation is, that he managed to convey to his pupils something of his own abundant energy and vitality, and that by the rapid transitions from one subject to another, and by his system of "problem papers," he maintained their minds in the condition of alertness which was demanded by examinations of the prevailing type.

Routh's fame as a teacher ought not, however, to be allowed to overshadow the real services which he rendered to mathematical physics, as a writer and an investigator. He was the author of a series of dynamical text-books which, continually improved in successive editions, have attained a very high degree of excellence. It is of interest to mention that some ten years ago these came under the notice of Prof. Felix Klein, of Göttingen, and excited his warm admiration. With his accustomed energy he promoted a translation of the most important, the "Rigid Dynamics," into German, as a corrective to the over abstract treatment of dynamical subjects which prevailed on the continent. Lastly it is to be recorded that as an original writer on the mechanics of systems endowed with "cyclic motions" he stands on a level with such men as Helmholtz and Lord Kelvin, who independently worked on the same subject. To the former he was probably unknown, but from the latter, as well as from Lord Rayleigh, he met with generous and emphatic appreciation. Some of the more important of his researches were embodied in the Adams Prize Essay "On Stability of Motion," published in 1877. He was elected a Fellow of the

experienced by a liquid film when stretched and kept at a constant temperature gives a limit to the size of the molecules of water. If a film of a two-hundred-millionth of a centimetre thick can exist as liquid at all, it is certain there cannot be many molecules in its thickness. 'I am writing' he says 'a short sketch of those of the results of Maxwell and Clausius which I use in it, to form part of an article on the Size of Atoms for *Nature*.'

Lord Kelvin's remains lie by the side of those of Newton in Westminster Abbey. At the invitation of the University of Glasgow, your President followed the coffin to the grave.

H. B. D.

By the death of Sir WILLIAM HENRY PERKIN, D.Sc., LL.D., Ph.D., Dr. Ing., F.R.S, which occurred suddenly at his residence at Harrow on July 14th, 1907, the Society lost one of its most distinguished Honorary Members (he was elected April 26, 1892) and Organic Chemistry one whose brilliant researches, both in pure and applied Science, entitle him to inclusion among those giants of the last century to whose genius the modern science of Organic Chemistry is due.

Sir William Perkin was born in London on March 12th, 1838, and received his general education at the City of London School. In 1853, at the early age of 15, he entered the Royal College of Chemistry as a student under Hofmann, who two years later appointed him his assistant.

It is probably given to few men to live to see the results of half a century of development of a discovery made by them, and the meeting at the Royal Institution on Thursday, July 26th, 1906, will long remain unique in the Annals of Science in that it represented the homage of the Scientific World to a man who just fifty years before had planted the seed of a new industry, seed, moreover, which was destined in this relatively short period of time to yield so abundant and rich a harvest as not

only to revolutionise the then existing methods of dyeing and dye manufacture, but at the same time to open up a new sphere of investigation by which our knowledge of the compounds of carbon has been vastly increased.

The story of the discovery of mauve, the first coal-tar colouring matter, has been fully told by Perkin himself in the Hofmann Memorial Lecture which he delivered before the Chemical Society in 1896. During the time that he was assistant to Hofmann he had fitted up for himself a laboratory at his father's house in Shadwell in order that he might be able to work in the evenings and during the vacations. It was here that during the Easter vacation of 1856 he was led to investigate the action of potassium dichromate on allyl toluidine in the hope of preparing synthetically the alkaloid quinine. In order to understand the reason which prompted Perkin to try this experiment, it must be remembered that at that time the conception of isomerism had not been evolved, and it was confidently believed that a synthetical product if of the same empirical formula would prove to be identical with a natural product. Hence when Hofmann, in the Report of the Royal College of Chemistry for 1849, referred to the fact that the hydrocarbon naphthalene could be converted by a series of chemical reactions into a crystalline alkaloid naphthalidine (= α-naphthylamine of to-day) he pointed out that this substance differed from quinine only by the elements of two molecules of water, and quaintly adds "we cannot of course expect to induce the water to enter merely by placing it in contact, but a happy experiment may attain this end by the discovery of an appropriate metamorphic process."

In this way Perkin was led to try the action of an oxidising agent on allyltoluidine in the hope that the following reaction would ensue:—

$$2(C_{10}H_{13}N) + 3O = C_{20}H_{24}O_2N_2 + H_2O.$$

allyltoluidine quinine

In the light of our present knowledge it is hardly necessary to say that no quinine was formed in this reaction, but only, to

use Perkin's own words, "a dirty reddish-brown precipitate," and, he continues, "unpromising though this result was I was interested in the reaction, and thought it desirable to treat a more simple base in the same manner. Aniline was selected, and its sulphate was treated with potassium dichromate; in this instance a black precipitate was obtained, and on examination this precipitate was found to contain the colouring matter since so well known as aniline purple or mauve."

The further story of the successful introduction of this laboratory product into commerce is also told by Perkin in the Hofmann Memorial Lecture, and it illustrates to a wonderful degree the genius and resourceful skill of this remarkable man. Even at the present day, with all our knowledge of chemical reactions and with all the elaborate factory methods at our disposal, it is an arduous task to adapt a laboratory experiment to commercial conditions, yet this man, met at every step by appalling difficulties and discouragements, was able to overcome them all and to found practically by himself a new and mighty industry; well might he say "it was all pioneering work."

It is impossible in the short space at our disposal to deal with the many discoveries with which Perkin enriched the Science of Organic Chemistry, but, if for no other reason, he must ever rank amongst the greatest teachers of science, because he was the first to show that pure science forms the most fertile field for technical and commercial progress. The value of this teaching cannot be overestimated, for it showed once and for all that the old and harmful superstition that industry could only learn from the practice and experience of daily life was hopelessly wrong.

Although Perkin left the Royal College of Chemistry in October, 1856, much against the advice of Hofmann, in order to carry out the manufacture of mauve, on the commercial scale, yet he never allowed his arduous technical duties to interfere with his research in pure chemistry. Thus only a year after starting the works at Greenford, that is in 1858, he published

his paper with Duppa on the constitution of glycocoll. In fact during the whole of the period of his association with the industry, that is, until 1874, he was directing and installing operations on the technical scale, whilst at the same time conducting original research in other branches of organic chemistry.

In 1868 Graebe and Liebermann settled the constitution of alizarine, the colouring matter of the madder root, by preparing it synthetically from anthracene, but the process they used was too costly for technical purposes. In the following year the method by which this, the first natural dye-stuff to be prepared synthetically, is at the present day manufactured was discovered and patented simultaneously by Caro, Græbe, and Liebermann in Germany and by Perkin in England. As a matter of fact the German patent of Caro, Graebe, and Liebermann bears the date June 25th, 1869; the English patent of Perkin the date June 26th, 1869.

The manufacture of artificial alizarine was carried out by Perkin at Greenford until 1873, when the works were sold to Messrs. Brooke, Simpson, and Spiller. The factory was subsequently transferred to the present British Alizarine Company who removed the manufacture from Greenford to Silvertown.

In 1874 Perkin retired from the industry and devoted the remainder of his life to the prosecution of research in pure chemistry. His work during this portion of his life is thus described by Professor J. W. Brühl, of Heidelberg, in the letter sent by him on the occasion of the Coal-tar Jubilee. "And, finally, at an age when most men would have retired on such well-earned laurels, you undertook once more an altogether new and wide-embracing task. Availing yourself of the marvellous discovery of your great countryman, Michael Faraday, you undertook to investigate the relations between the chemical composition of bodies and their magnetic circular polarization—that is to say, one of the general properties of all matter. . . . You created a new branch of science, taught us how from the magnetic rotation conclusions can be drawn as to the chemical

structure of bodies, and showed that the magnetic rotation allows us to draw comprehensive and certain conclusions as to the chemical constitution of substances."

Sir William Perkin literally died in harness, and only a few months before his death he read his last paper before the Chemical Society in which he described the determination of the magnetic rotation of van Romburgh's hexatriene. The numbers obtained clearly showed the presence of three conjugated double linkages in benzene, thus indicating the correctness of the Kekulè formula.

In conclusion, one cannot do better than quote a sentence from the address of the Society of Dyers and Colourists, presented on the occasion of the Jubilee, which ran, "You have given to science the allegiance of a noble life, and have not allowed the seduction of wealth to abate the loyalty of your devotion to truth and knowledge. For this example the age owes you unstinted thanks."

J. F. T.

EDWARD JOHN ROUTH was born at Quebec in 1831. He was educated at University College, London, and at Peterhouse, Cambridge, and graduated as Senior Wrangler in 1854. Among his contemporaries was James Clerk Maxwell, who stood second in the same list. He soon afterwards entered on that extraordinarily successful career of mathematical tuition which terminated only in 1888, when a large number of his old pupils, including men of the highest distinction in science and in the law, joined in the presentation of a portrait by Herkomer to Mrs. Routh.

To the outside public Routh's name is chiefly associated with the particular system of mathematical training which prevailed so long at Cambridge, and is only now coming to an end. It is probable, indeed, that he was in the first instance chiefly concerned with mathematics as an educational instrument. He attached to it the highest importance from this

of the fibres he had not then been able to identify, but it resembled "China grass."

Professor DIXON said he had examined some of the powder into which the body had decomposed, and found that it contained an organic salt of sodium, and that the solution in water gave a lather when shaken; it was in fact a kind of soap.

A number of beetle shells found in the coffin were exhibited, and Dr. Hoyle said that they were identical with those of a modern species, *Gibbium scotias*, which fed on hair and various fibres.

Annual Report of the Council, April, 1908.

The Society began the session with an ordinary membership of 150. During the present session 19 new members have joined the Society. Thirteen resignations have been received, and there has been one death, *viz.:* Mr. JAMES WHITEHEAD. This leaves on the roll 155 ordinary members. The Society has also lost, by death, 7 honorary members, *viz.:* Sir BENJAMIN BAKER, K.C.M.G., F.R.S.; Dr. ALEXANDER BUCHAN, F.R.S.; Lord KELVIN, O.M., G.C.V.O., F.R.S.; Sir W. H. PERKIN, F.R.S.; Dr. E. J. ROUTH, F.R.S.; Dr. HENRY C. SORBY, F.R.S.; and Professor C. A. YOUNG. Memorial notices of these gentlemen appear at the end of this report.

The average attendance at the meetings was 32 as compared with 24 for the session 1906-07.

The Society commenced the session with a balance in hand of £214. 16s. 7d., from all sources, this amount being made up of the following balances :—

At the credit of	General Fund.....	£26	16	0
„ „	Wilde Endowment Fund...	71	12	9
„ „	Joule Memorial Fund.....	80	6	2
„ „	Dalton Tomb Fund	36	1	8
		£214	16	7

The total balance in hand at the close of the session amounted to £208. 19s. 6d., and the amounts standing at the credit of the separate accounts, on the 31st March, 1908, are the following :—

At the credit of	General Fund.....	£17	0	9
„ „	Wilde Endowment Fund...	66	18	1
„ „	Joule Memorial Fund	88	1	0
„ „	Dalton Tomb Fund	36	19	8
Balance 31st March, 1908.....		£208	19	6

The Wilde Endowment Fund, which is kept as a separate banking account shows a balance of £66. 18s. 1d., in its favour, as against £71. 12s. 9d., at the beginning of the financial year, the receipts from the invested funds being the same as last year.

During the session improvements in the lighting and ventilating of the Library were carried out, and the floor of the caretakers' room was covered with linoleum. The cost, amounting to £26. 2s. 5d., has been charged to the Wilde Endowment Fund.

The Librarian reports that during the session 897 volumes have been stamped, catalogued and pressmarked, 804 of these being serials, and 93 separate works. There have been written 415 catalogue cards, 292 for serials, and 123 for separate works. The total number of volumes catalogued to date is 30,673 for which 10,859 cards have been written.

Satisfactory use is made of the library for reference purposes, but the number of volumes consulted is not recorded. During the session, 205 volumes have been borrowed from the library, as compared with 176 in the previous session.

Some attention has continued to be paid to the completion of sets, 42 volumes or parts having been obtained, which complete three sets, and partly complete three. Of these, 8 volumes were purchased, and the rest were presented by the societies publishing them.

A larger amount of binding than usual has been done this session, 200 volumes having been bound in 183.

A record of the accessions to the library shows that, from April, 1907, to March, 1908, 812 serials and 110 separate works were received, a total of 922 volumes. The donations during the session (exclusive of the usual exchanges) amount to 106 volumes and 89 dissertations; 8 volumes have been purchased (in addition to the periodicals on the regular subscription list).

During the past session the Society has arranged to exchange publications with the following:—The University of New Mexico, Albuquerque; the Naturwissenshaftlicher Verein in Hamburg, complete sets of whose *Verhandlungen* and *Abhandlungen* have been acquired; and the Agricultural Research Institute, Pusa, Bengal.

The *Chemical Abstracts*, published by the American Chemical Society, have been added to the list of periodicals subscribed for.

The publication of the *Memoirs and Proceedings* has been continued under the supervision of the Editorial Committee.

The Society is indebted to the following gentlemen for the undermentioned gifts:—

Dr. F. W. Jordan, for engraved portraits of John Dalton, and of Thomas Young formerly Secretary of the Royal Society. The latter is from a painting by Sir Thomas Lawrence.

Professor H. B. Dixon, F.R.S., for copies of the following works by Robert Boyle:—"Hydrostatical Paradoxes," 1666; "New Experiments touching the Relation betwixt Flame and Air," 1672; "Origine of Formes and Qualities," 1667; and "Imperfection of the Chymist's Doctrine of Qualities," 1676.

Professor A. Schuster, F.R.S., for a set of the Publications of the International Seismological Association.

Mr. F. Nicholson, for defraying the cost of repairing and re-gilding the portrait of the Rev. George Walker, which is hung in the Council Room.

The Council awarded the Wilde Medal for 1908 to Dr. Joseph Larmor, Sec.R.S., Lucasian Professor of Mathematics in the University of Cambridge, for his researches on the constitution of matter and ether.

MANCHESTER LITERARY AN

Dr. *Arthur McDougall, Treasurer, in Account with t*

			£	s.	d.	£	s.
To Cash in hand, 1st April, 1907						107	2
To Members' Subscriptions:—							
Half Subscriptions, 1906-07,	2 at £1 1s. 0d.		2	2	0		
,, ,, 1907-08,	18 ,, ,,		18	18	0		
Subscriptions:— 1900-01,	1 ,, £2. 2s. 0d.		2	2	0		
,, 1901-02,	1 ,, ,,		2	2	0		
,, 1902-03,	1 ,, ,,		2	2	0		
,, 1903-04,	1 ,, ,,		2	2	0		
,, 1904-05,	4 ,, ,,		8	8	0		
,, 1905-06,	8 ,, ,,		16	16	0		
,, 1906-07,	18 ,, ,,		37	16	0		
,, 1907-08,	115 ,, ,,		241	10	0		
,, 1908-09,	2 ,, ,,		4	4	0		
						338	2
To Transfers from the Wilde Endowment Fund						83	12
To Sale of Publications						32	12
To Dividends:—							
Natural History Fund			58	3	8		
Joule Memorial Fund			7	7	2		
						65	10
To Income Tax Refunded:—							
Natural History Fund				11			
Joule Memorial Fund			0	7	8		
						3	9
To A. McDougall for coloured plate						17	5
To H. Sidebottom for plates						5	5
						£652	18

NATURAL HISTORY

	£	s.	d.
To Balance 1st April, 1907			2 13 11
To Dividends on £1,225 Great Western Railway Company's Stock			58 3 8
To Remission of Income Tax, 1907			3 1 4
			£63 18 11

JOULE MEMORIAL

	£	s.	d.
To Balance, 1st April, 1907			80 6 2
To Dividends on £258 Loan to Manchester Corporation			7 7 2
To Remission of Income Tax, 1907			0 7 8
			£88 1 0

WILDE ENDOWMENT

	£	s.	d.
To Balance 1st April, 1907			71 12 9
To Dividends on £7,500 Gas Light and Coke Company's Ordinary Stock			313 10 0
To Remission of Income Tax, 1907			16 10 0
To Bank Interest			1 13 2
			£403 5 11

DALTON TOMB

	£	s.	d.
To Balance, 1st April, 1907			36 1 8
To Bank Interest			0 18 0
			£36 19 8

PHILOSOPHICAL SOCIETY.

Society, from 1st April, 1907, to 31st March, 1908. **Cr.**

	£ s. d.	£. s. d.
By Charges on Property :—		
Chief Rent (Income Tax deducted)	12 5 4	
Income Tax on Chief Rent	0 12 11	
Insurance against Fire	13 17 6	
By House Expenditure :—		26 15 9
Coals, Gas, Electric Light, Water, &c.	42 4 7	
Tea, Coffee, &c., at Meetings	17 15 1½	
Cleaning, Sweeping Chimneys, &c.	4 10 10½	
Replacements of mantles, burners, towels, etc., and repair of locks	2 17 10	
By Administrative Charges :—		67 8 5
Housekeeper	65 0 0	
Postages, and Carriage of Parcels and of "Memoirs"	37 6 10½	
Stationery, Cheques, Receipts, and Engrossing	9 6 0	
Printing Circulars, Reports, &c.	14 17 6	
Extra attendance at Meetings, and during housekeeper's holidays	3 8 6	
Insurance against Liability	1 4 9	
Miscellaneous Expenses	0 17 8½	
		132 1 4
By Excess subscription refunded	.. ..	1 1 0
By Legal Expenses	.. ..	5 0 6
By Publishing :—		
Printing "Memoirs and Proceedings"	190 7 7	
Illustrations for "Memoirs" (except Nat. Hist. papers)	20 8 8	
Binding "Memoirs"	2 15 6	
		213 11 9
By Library :—		
Books and Periodicals (except those charged to Natural History Fund) ..	33 17 4	
Periodicals formerly subscribed for by the Microscopical and Natural History Section	4 1 3	
		37 18 7
By Natural History Fund :—		
(Items shown in the Balance Sheet of this Fund below)	.. ..	63 18 11
By Joule Memorial Fund :—		
(No Expenditure this Session)	.. ..	0 0 0
By Balance at Williams Deacon's Bank, 1st April, 1908	95 1 9	
,, ,, in Treasurer's hands	10 0 0	
		105 1 9
		£652 18 0

FUND, 1907—1908. (Included in the General Account, above.)

	£ s. d.
By Natural History Books and Periodicals	47 1 11
By illustrations for papers on Nat. Hist. in "Memoirs"	16 17 0
	£63 18 11

FUND, 1907—1908. (Included in the General Account, above.)

	£ s. d.
No expenditure this Session).	
By Balance, 1st April, 1908	88 1 0
	£88 1 0

FUND, 1907—1908.

	£ s. d.
By Assistant Secretary's Salary, April, 1907, to March, 1908	150 0 0
By Maintenance of Society's Library :—	
Binding and Repairing Books	29 9 8
Periodicals to complete sets	7 0 8
	36 10 4
By Repairs and Improvements to Society's Premises	15 15 1
By Extension and Improvement of Electric Lighting	12 16 1
By Crockery and Stepladder	1 17 10
By Honorarium to Wilde Lecturer, 1908	15 15 0
By Wilde Medal	19 19 0
By Transfers to Society's Funds	83 12 0
By Cheque Book	0 2 6
By Balance at District Bank, 1st April, 1908	66 18 1
	£403 5 11

FUND, 1907—1908.

	£ s. d.
No Expenditure this Session).	
By Balance at Manchester and Salford Savings Bank, 1st April, 1908	36 19 8
	£36 19 8

THE COUNCIL
AND MEMBERS
OF THE
MANCHESTER
LITERARY AND PHILOSOPHICAL SOCIETY.

(Corrected to July 8th, 1908.)

President.

H. B. DIXON, M.A., F.R.S., F.C.S.

Vice-Presidents.

SIR WILLIAM H. BAILEY, M.I.Mech.E.
FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.
W. E. HOYLE, M.A., D.Sc., F.R.S.E.
THOMAS THORP, F.R.A.S.

Secretaries.

F. W. GAMBLE, D.Sc., F.R.S.
R. L. TAYLOR, F.C.S., F.I.C.

Treasurer.

ARTHUR McDOUGALL, B.Sc.

Librarian.

C. L. BARNES, M.A.

Other Members of the Council.

FRANCIS NICHOLSON, F.Z.S.
CHARLES BAILEY, M.Sc., F.L.S.
ERNEST F. LANGE, F.C.S., M.I.Mech.E.
WILLIAM J. POPE, F.R.S., F.C.S.
HORACE LAMB, M.A., LL.D., D.Sc., Sc.D., F.R.S.
ERNEST RUTHERFORD, F.R.S.

Assistant Secretary and Librarian.

A. P. HUNT, B.A.

ORDINARY MEMBERS.

Date of Election.

1901, Dec. 10. Adamson, Harold. *Oaklands Cottage, Godley, near Manchester.*

1902, Mar. 18. Allen, J. Fenwick. 147, *Withington Road, Whalley Range, Manchester.*

1870, Dec. 13. Angell, John, F.C.S., F.I.C. 6, *Beaconsfield, Derby Road, Withington, Manchester.*

1865, Nov. 14. Bailey, Charles, M.Sc., F.L.S. *Atherstone House, North Drive, St. Annes-on-the-Sea, Lancs.*

1888, Feb. 7. Bailey, Alderman Sir William H., M.I.Mech.E. *Sale Hall, Sale, Cheshire.*

1895, Jan. 8. Barnes, Charles L., M.A. 8, *Swinton Avenue, Chorlton-on-Medlock, Manchester.*

1903, Oct. 20. Barnes, Jonathan, F.G.S. *South Cliff House*, 301, *Great Clowes Street, Higher Broughton, Manchester.*

1908, Feb. 11. Bateman, H., B.A., Reader in Mathematical Physics in the University of Manchester. *The University, Manchester.*

1895, Mar. 5. Behrens, Gustav. *Holly Royde, Withington, Manchester.*

1898, Nov. 29. Behrens, Walter L. 22, *Oxford Street, Manchester.*

1868, Dec. 15. Bickham, Spencer H., F.L.S. *Underdown, Ledbury.*

1901, Nov. 12. Bles, Abraham J. S. *Palm House, Higher Broughton, Manchester.*

1896, Oct. 6. Bowman, F.H., D.Sc., F.R.S.E. 4, *Albert Square, Manchester.*

1875, Nov. 16. Boyd, John. *Barton House*, 11, *Didsbury Park, Didsbury, Manchester.*

1889, Oct. 15. Bradley, Nathaniel, F.C.S. *Sunnyside, Whalley Range, Manchester.*

1861, April 2. Brogden, Henry, F.G.S., M.I.Mech.E. *Hale Lodge, Altrincham, Cheshire.*

1889, April 16. Brooks, Samuel Herbert. *Slade House, Levenshulme, Manchester.*

1860, Jan. 24. Brothers, Alfred. *Handforth, near Manchester.*

1886, April 6. Brown, Alfred, M.A., M.D. *Sandycroft, Higher Broughton, Manchester.*

1889, Jan. 8. Brownell, Thomas William, F.R.A.S. 64, *Upper Brook Street, Manchester.*

1889, Oct. 15. Budenberg, C. F., M.Sc., M.I.Mech.E. *Bowdon Lane, Marple, Cheshire.*

Date of Election.

1906, Feb. 27. Burton, Joseph, A.R.C.S., Dublin. *Tile Works, Clifton Junction, near Manchester.*

1894, Nov. 13. Burton, William, F.C.S. *The Hollies, Clifton Junction, near Manchester.*

1904, Oct. 18. Campion, George Goring, L.D.S. 264, *Oxford Street, Manchester.*

1907, Jan. 15. Carpenter, H. C. H., M.A., Ph.D., Professor of Metallurgy in the University of Manchester. 11, *Oak Road, Withington, Manchester.*

1899, Feb. 7. Chapman, D. L., M.A., Fellow of Jesus College, Oxford. *Jesus College, Oxford.*

1901, Nov. 26. Chevalier, Reginald C., M.A., Mathematical Master at the Manchester Grammar School. 103, *Clyde Road, West Didsbury, Manchester.*

1907, Nov. 26. Clayton, Robert Henry, B.Sc., Chemist. *Woodleigh, Blackfield Lane, Kersal, Manchester.*

1902, Nov. 4. Clerk, Dugald, F.R.S., M.Inst.C.E., F.C.S. 18, *Southampton Buildings, Chancery Lane, London, W.C.*

1901, Nov. 12. Coignou, Caroline, M.A., Science Mistress at the Manchester High School for Girls. 60, *Cecil Street, Greenheys, Manchester.*

1895, April 30. Collett, Edward Pyemont. 8, *St. John Street, Manchester.*

1903, Oct. 20. Core, William Hamilton, M.Sc. *Groombridge House, Withington, Manchester.*

1906, Oct. 30. Coward, H. F., M.Sc., Assistant Lecturer in Chemistry in the University of Manchester. *Hulme Hall, Plymouth Grove, Manchester.*

1906, Nov. 27. Coward, Thomas Alfred, F.Z.S. *Brentwood, Bowdon, Cheshire.*

1895, Nov. 12. Crossley, W. J., M.I.Mech.E. *Openshaw, Manchester.*

1901, Nov. 26. Darbishire, Francis V., B.A., Ph.D., Demonstrator and Analyst at the South Eastern Agricultural College. *The College, Wye, Kent.*

1895, April 9. Dawkins, W. Boyd, M.A., D.Sc., F.R.S., late Professor of Geology in the Victoria University of Manchester. *Fallowfield House, Fallowfield, Manchester.*

1894. Mar. 6. Delépine, A. Sheridan, M.B., B.Sc., Professor of Pathology in the Victoria University of Manchester. *The University, Manchester.*

Date of Election.

1887, Feb. 8. Dixon, Harold Baily, M.A., M.Sc., F.R.S., F.C.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1907, Oct. 29. Doggett, Captain Arthur, Works Secretary, Vulcan Locomotive Works, Newton-le-Willows. 48, *Gilda Brook Road, Eccles.*

1905, Jan. 10. Duffield, W. Geoffrey, B.A., M.Sc., Research Fellow in the University of Manchester. *The University, Manchester.*

1906, Jan. 30. Dunkerley, Stanley, D.Sc., Professor of Engineering in the University of Manchester. *The University, Manchester.*

1906, Oct. 30. Edgar, E. C., D.Sc., Assistant Lecturer and Demonstrator in Chemistry in the University of Manchester. *Dalton Hall, Victoria Park, Manchester.*

1902, May 13. Ellison, Robert William. *'Brookside,' Crofts Bank Road, Urmston, Manchester.*

1906, Nov. 13. Fisher, Kenneth, B.A. Oxon., Ph.D. Jena, Research Student in the University of Manchester. *The Lilacs, Timperley, Cheshire.*

1907, Nov. 26. Flatters, Abraham, F.R.M.S. *Syddal Cottage, Bramhall, Cheshire.*

1908, Mar. 10. Fowler, W. H., M.Inst.C.E. *Sale Lodge, Sale, Cheshire.*

1908, Jan. 28. Fox, Thomas William, M.Sc.Tech., Professor of Textiles in the School of Technology, Manchester. 15, *Clarendon Crescent, Eccles.*

1898, Nov. 29. Gamble, F. W., D.Sc., F.R.S., Assistant Director of the Zoological Laboratories in the Victoria University of Manchester. *The University, Manchester,* and *Heathwaite, Bramhall Lane, Stockport.*

1896, Nov. 17. Gordon, Rev. Alexander, M.A. *Summerville, Victoria Park, Manchester.*

1907, Oct. 15. Gravely, F. H., B.Sc., Assistant Lecturer and Demonstrator in Zoology in the University of Manchester. *Dalton Hall, Victoria Park, Manchester.*

1907, Oct. 29. Gwyther, Reginald Francis, M.A., Secretary to the Joint Matriculation Board. 15, *Booth Avenue, Withington, Manchester.*

Date of Election.

1902, April 29. Herbert, Arthur M., B.A. *Frankwyn, Hale, Cheshire.*

1905, Oct. 17. Hewitt, Charles Gordon, M.Sc., Lecturer in Economic Zoology in the Victoria University of Manchester. *The University, Manchester.*

1902, Jan. 7. Hewitt, David B., M.D. *Grove Mount, Davenham, Cheshire.*

1907, Oct. 15. Hickling, H. George A., B.Sc., Assistant Lecturer and Demonstrator in Geology in the University of Manchester. 50, *Lancaster Road, Fallowfield, Manchester.*

1895, Mar. 5. Hickson, Sydney J., M.A., D.Sc., F.R.S., Professor of Zoology in the Victoria University of Manchester. *The University, Manchester.*

1884, Jan. 8. Hodgkinson, Alexander, M.B., B.Sc. 18, *St. John Street, Manchester.*

1906, Oct. 16. Holmes, H. T., M.A. (Cantab.). *Eastnor, 77, Wellington Road, Heaton Chapel.*

1905, Nov. 14. Holt, Alfred, M.A., Research Fellow of the University of Manchester. *Crofton, Aigburth, Liverpool.*

1898, Nov. 29. Hopkinson, Alfred, K.C., M.A., LL.D., Vice-Chancellor of the Victoria University of Manchester. *Fairfield, Victoria Park, Manchester.*

1896, Nov. 3. Hopkinson, Edward, M.A., D.Sc., M.Inst.C.E. *Ferns, Alderley Edge, Cheshire.*

1889, Oct. 15. Hoyle, William Evans, M.A., D.Sc., F.R.S.E., Director of the Manchester Museum. *The University, Manchester.*

1907, Oct. 15. Hübner, Julius, M.Sc.Tech., F.I.C., Lecturer in the Faculty of Technology in the University of Manchester. *Ash Villa, Cheadle Hulme, Cheshire.*

1900, Oct. 16. Hutton, R. S., D.Sc., Lecturer on Electro-Chemistry in the Victoria University of Manchester. 81, *Clarkehouse Road, Sheffield.*

1899, Oct. 17. Ingleby, Joseph, M.I.Mech.E. *Summer Hill, Pendleton, Manchester.*

1901, Nov. 26. Jackson, Frederick. 14, *Cross Street, Manchester.*

1870, Nov. 1. Johnson, William H., B.Sc. *Woodleigh, Altrincham.*

1878, Nov. 26. Jones, Francis, M.Sc., F.R.S.E., F.C.S. *Manchester Grammar School*, and *Beaufort House, Alexandra Park, Manchester.*

Date of Election.

1886, Jan. 12. Kay, Thomas. *Moorfield, Stockport, Cheshire.*

1895, Nov. 12. Kirkman, William Wright. *The Grange, Timperley, Cheshire.*

1903, Feb. 3. Knecht, Edmund, Ph.D., Professor of Tinctorial Chemistry at the Municipal School of Technology, Manchester. *Beech Mount, Marple, Cheshire.*

1904, Oct. 18. Knowles, H. B., M.A., Principal of the Royal Technical Institute, Salford. *'Norcote,' Swinton Park, Salford.*

1902, Feb. 4. Kolp, Noah. *Woodthorpe, Victoria Park, Manchester.*

1893, Nov. 14. Lamb, Horace, M.A., LL.D., D.Sc., Sc.D., F.R.S., Professor of Mathematics in the Victoria University of Manchester. 6, *Wilbraham Road, Fallowfield, Manchester.*

1902, Jan. 7. Lange, Ernest F., M.I.Mech.E., F.C.S. *Fairholme,* 3, *Willow Bank, Fallowfield, Manchester.*

1904, Mar. 15. Lea, Arnold W. W., M.D. 246, *Oxford Road, Manchester.*

1903, Nov. 17. Leigh, Charles W. E., Librarian of the University. *The University, Manchester.*

1907, Oct. 29. Leigh, Harold Shawcross. *Brentwood, Worsley.*

1902, Nov. 4. Leigh, Joseph Egerton. *The Towers, Didsbury, Manchester.*

1908, Jan. 14. Littlewood, J. E., B.A., Richardson Lecturer in Mathematics in the University of Manchester. *The University, Manchester.*

1902, Jan. 7. Longridge, Michael, M.A., M.Inst.C.E. *Linkvretten, Ashley Road, Bowdon, Cheshire.*

1857, Jan. 27. Longridge, Robert Bewick, M.I.Mech.E. *Yew Tree House, Tabley, Knutsford, Cheshire.*

1866, Nov. 13. McDougall, Arthur, B.Sc. *Lyndhurst, The Park, Buxton.*

1905, Oct. 31. McNicol, Mary, M.Sc., Research Scholar in the Victoria University of Manchester. 182, *Upper Chorlton Road, Manchester.*

1904, Nov. 1. Makower, Walter, B.A., B.Sc., Lecturer in Physics in the University of Manchester. 214, *Upper Brook Street, Manchester.*

1902, Mar. 4. Mandleberg, Goodman Charles. *Redclyffe, Victoria Park, Manchester.*

1875, Jan 26. Mann, J. Dixon, M.D., F.R.C.P. (Lond.), Professor of Medical Jurisprudence in the Victoria University of Manchester. 16, *St. John Street, Manchester.*

Date of Election.

1901, Dec. 10. Massey, Herbert. *Ivy Lea, Burnage, Didsbury, Manchester.*

1864, Nov. 1. Mather, Sir William, M.Inst.C.E., M.I.Mech.E. *Iron Works, Salford.*

1873, Mar. 18. Melvill, James Cosmo, M.A., D.Sc., F.L.S. *Meole-Brace Hall, Shrewsbury.*

1881, Oct. 18. Mond, Ludwig, D.Sc., Ph.D., F.R.S., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1894, Feb. 6. Mond, Robert Ludwig, M.A., F.R.S.E., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1903, Oct. 20. Moore, Frederick Craven, M.D., M.Sc., 61, *Ardwick Green, Manchester.*

1899 Mar. 7. Morris, Edgar F., M.A., F.C.S. *Grey House, Barrington Road, Altrincham, Cheshire.*

1902, Feb. 18. Moss, William E., B.A. *C/o Messrs. Davies, Benachi & Co., 7, Rumford Street, Liverpool.*

1908, Jan. 28. Myers, William, Lecturer in Textiles in the School of Technology, Manchester. *Stone Edge, Marple.*

1873, Mar. 4. Nicholson, Francis, F.Z.S. *The Knoll, Windermere, Westmorland.*

1900, April 3. Nicolson, John T., D.Sc., Professor of Engineering at the Municipal School of Technology, Manchester. *Nant-y-Glyn, Marple, Cheshire.*

1884, April 15. Okell, Samuel, F.R.A.S. *Overley, Langham Road, Bowdon, Cheshire.*

1907, Oct. 29. Osborn, Theodore George Bentley, B.Sc., Lecturer in Economic Botany in the University of Manchester. 2, *Deyne Avenue, Rusholme, Manchester.*

1901, Nov. 26. Paine, Standen. *Devisdale, Bowdon, Cheshire.*

1892, Nov. 15. Perkin, W. H., Ph.D., M.Sc., F.R.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1901, Oct. 29. Petavel, J. E., B.A., D.Sc., F.R.S., Professor of Engineering in the University of Manchester. *The University, Manchester.*

1885, Nov. 17. Phillips, Henry Harcourt, F.C.S. *Lynwood, Turton, nr. Bolton, Lancs.*

1902, Oct. 21. Pope, W. J., F.R.S., F.C.S., Professor of Chemistry at the Municipal School of Technology, Manchester. *Corchester, Bramhall, Cheshire.*

Date of Election.

1903, Dec. 15. Prentice, Bertram, Ph.D., D.Sc., Lecturer in Chemistry, Royal Technical Institute, Salford. *Isca Mount, Manchester Road, Swinton.*

1904, Feb. 2. Radford, Catherine, B.Sc. 31, *Cawdor Road, Fallowfield, Manchester.*

1900, Feb. 20. Ragdale, John R. *The Beeches, Whitefield, near Manchester.*

1901, Dec. 10. Ramsden, Herbert, M.D. (Lond.), M.B., Ch.B. (Vict.). *Sunnyside, Dobcross, near Oldham, Lancs.*

1888, Feb. 21. Rée, Alfred, Ph.D., F.C.S. 15, *Mauldeth Road, Withington, Manchester.*

1906, Oct. 30. Renold, Charles G., Engineer. 35, *Mabfield Road, Fallowfield, Manchester.*

1869, Nov. 16. Reynolds, Osborne, M.A., LL.D., F.R.S., M.Inst.C.E., Beyer Research Professor of Civil and Mechanical Engineering in the University of Manchester. 19, *Ladybarn Road, Fallowfield, Manchester.*

1880, Mar. 23. Roberts, D. Lloyd, M.D., F.R.S.E., F.R.C.P. (Lond.). *Ravenswood, Broughton Park, Manchester.*

1897, Oct. 19. Rothwell, William Thomas. *Heath Brewery, Newton Heath, near Manchester.*

1907, Oct. 15. Rutherford, Ernest, M.A., F.R.S., Langworthy Professor of Physics in the University of Manchester. 17, *Wilmslow Road, Withington, Manchester.*

1905, Oct. 31. Saxelby, Edith Mary, B.Sc., Research Scholar in the Victoria University of Manchester. 3, *Alexandra Road South, Alexandra Park, Manchester.*

1873, Nov. 18. Schuster, Arthur, Sc.D., Ph.D., F.R.S., F.R.A.S., Honorary Professor of Physics in the Victoria University of Manchester. *Kent House, Victoria Park, Manchester.*

1898, Jan. 25. Schwabe, Louis. *Hart Hill, Eccles Old Road, Pendleton, Manchester.*

1908, May 12. Scott, Charles Prestwich, M.A. *The Firs, Fallowfield, Manchester.*

1890, Nov. 4. Sidebotham, Edward John, M.A., M.B., M.R.C.S. *Erlesdene, Bowdon, Cheshire.*

1903, April 28. Sidebottom, Henry. *The Hall Cottage, Cheadle Hulme, near Stockport.*

1907, Oct. 15. Simonsen, J. L., M.Sc., Assistant Lecturer in Chemistry in the University of Manchester. 152, *Barlow Moor Road, West Didsbury, Manchester.*

Date of Election.

1906, Oct. 30. Smedley, Ida, D.Sc., Assistant Lecturer in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1906, Nov. 27. Smith, Norman, D.Sc., Assistant Lecturer in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1895, Nov. 12. Southern, Frank, B.Sc. 6, *Park Avenue, Timperley, Cheshire.*

1896, Feb. 18. Spence, David. *Lowood, Hindhead, Haslemere, R.S.O., Surrey.*

1901, Dec. 10. Spence, Howard. *Audley, Broad Road, Sale, Cheshire.*

1904, Nov. 1. Stansfield, Herbert, B.Sc., A.I.E.E. 20, *Every Street, Ancoats, Manchester.*

1897, Nov. 30. Stromeyer, C. E., M.Inst.C.E. *Steam Users' Association,* 9, *Mount Street, Albert Square, Manchester.*

1905, Nov. 1. Sutcliffe, William Henry, F.G.S. *Shore, Littleborough, Lancs.*

1895, April 9. Tatton, Reginald A., M.Inst.C.E. Engineer to the Mersey and Irwell Joint Committee. *Manor House, Chelford, Cheshire.*

1893, Nov. 14. Taylor, R. L., F.C.S., F.I.C. *Central School, Whitworth Street,* and 4, *St. Werburgh's Road, Chorlton-c.-Hardy, Manchester.*

1906, April 10. Thewlis, Councillor J. H. *Daisy Mount, Victoria Park, Manchester.*

1873, April 15. Thomson, William, F.R.S.E., F.C.S., F.I.C. *Royal Institution, Manchester.*

1896, Jan. 21. Thorburn, William, M.D., B.Sc. 2, *St. Peter's Square, Manchester.*

1896, Jan. 21. Thorp, Thomas, F.R.A.S. *Moss Bank, Whitefield, near Manchester.*

1899, Oct. 31. Thorpe, Jocelyn F., Ph.D., F.R.S., Lecturer and Demonstrator in Organic Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1899, Oct. 17. Todd, William Henry. *Greenfield, Flixton, near Manchester,* and *Sickle Street, corner of* 84, *Market Street.*

1873, Nov. 18. Waters, Arthur William, F.L.S., F.G.S. "*Alderley,*" *McKinley Road, Bournemouth.*

1906, Nov. 13. Watson, D. M. S., B.Sc. 466, *Moss Lane East, Manchester.*

Date of Election.

1892, Nov. 15. Weiss, F. Ernest, D.Sc., F.L.S., Professor of Botany in the Victoria University of Manchester. 30, *Brunswick Road, Withington, Manchester.*

1908, May 12. Welldon, Rt. Rev. J. E. C., D.D., Dean of Manchester. *The Deanery, Manchester.*

1907, Oct. 29. Whitehead, Thomas, B.Sc., Chemist to the Manchester Steam Users' Association. 89, *Kenworthy Street, Stalybridge.*

1901, Oct. 1. Wild, Robert B., M.D., M.Sc., M.R.C.P., Professor of Materia Medica and Therapeutics in the Victoria University of Manchester. *Broome House, Fallowfield, Manchester.*

1859, Jan. 25. Wilde, Henry, D.Sc., D.C.L., F.R.S. *The Hurst, Alderley Edge, Cheshire.*

1905, Oct. 31. Willis, Ethel G., M.A., B.Sc., Science Mistress, Manchester High School for Girls. 2, *Lancaster Road, Fallowfield, Manchester.*

1901, Nov. 26. Wilson, William, M.A. *Carron Vale*, 80, *Fitzwarren Street, Pendleton, Manchester.*

1907, Oct. 15. Winstanley, George H., F.G.S., M.I.M.E., Lecturer in Mining Engineering and Mine Surveying in the University of Manchester. *Wigshaw Grange, Culcheth, near Warrington.*

1903, Oct. 20. Wood, Harry Edwin, B.Sc., Assistant to the Government Meteorological Department, Johannesburg.

1905, Oct. 31. Woodall, Herbert J., A.R.C.S. 32, *Market Place, Stockport.*

1860, April 17. Woolley, George Stephen. *Victoria Bridge, Manchester.*

1863, Nov. 17. Worthington, Samuel Barton, M.Inst.C.E., M.I.Mech.E. *Mill Bank, Bowdon*, and 37, *Princess Street, Manchester.*

1865, Feb. 21. Worthington, Thomas, F.R.I.B.A. *Broomfield, Alderley Edge, Manchester.*

1895, Jan. 8. Worthington, Wm. Barton, B.Sc., M.Inst.C.E. *Kirkstyles, Duffield, near Derby.*

1897, Oct. 19. Wyatt, Charles H., M.A., *Chelford, Cheshire.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore life members:—

Bailey, Charles, M.Sc., F.L.S.
Bradley, Nathaniel, F.C.S.
Brogden, Henry, F.G.S.
Ingleby, Joseph, M.I.Mech.E.
Johnson, William H., B.Sc.
Worthington, Wm. Barton, B.Sc.

HONORARY MEMBERS.

Date of Election.

1892, April 26. Abney, Sir W. de W., K.C.B., D.Sc., F.R.S. *Rathmore Lodge, Bolton Gardens South, South Kensington, London, S.W.*

1892, April 26. Amagat, E. H., For. Mem. R.S., Memb. Inst. Fr. (Acad. Sci.), Examinateur à l'École Polytechnique. *Avenue d'Orléans, 19, Paris.*

1894, April 17. Appell, Paul, Membre de l'Institut, Professor of Theoretical Mechanics. *Faculté des Sciences, Paris.*

1892, April 26. Ascherson, Paul F. Aug., Professor of Botany. *Universität, Berlin.*

1889, April 30. Avebury, John Lubbock, Lord, D.C.L., LL.D., F.R.S. *High Elms, Down, Kent.*

1892, April 26. Baeyer, Adolf von, For. Mem. R.S., Professor of Chemistry. 1, *Arcisstrasse, Munich.*

1886, Feb. 9. Baker, John Gilbert, F.R.S., F.L.S. 3, *Cumberland Road, Kew.*

1888, April 17. Cannizzaro, Stanislao, For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Chemistry. *Reale Università, Rome.*

1889, April 30. Carruthers, William, F.R.S., F.L.S. 14, *Vermont Road, Norwood, London, S.E.*

1903, April 28. Clarke, Frank Wigglesworth, D.Sc. *United States Geological Survey, Washington, D.C., U.S.A.*

1866, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.S., F.R.A.S., Professor of Natural Philosophy. 3, *Bardwell Road, Banbury Road, Oxford.*

1892, April 26. Curtius, Theodor, Professor of Chemistry. *Universität, Kiel.*

1892, April 26. Darboux, Gaston, Membre de l'Institut, Professor of Geometry, Faculté des Sciences, Secrétaire perpétuel de l'Académie des Sciences. 36, *Rue Gay Lussac, Paris.*

1894, April 17. Debus, H., Ph.D., F.R.S. 4. *Schlangenweg, Cassel, Hessen, Germany.*

Date of Election.

1900, April 24. Dewar, Sir James, M.A., LL.D., D.Sc., F.R.S., V.P.C.S., Fullerian Professor of Chemistry. *Royal Institution, Albemarle Street, London, W.*

1892, April 26. Dohrn, Dr. Anton, For. Mem. R.S. *Zoologische Station, Naples.*

1892, April 26. Edison, Thomas Alva. *Orange, N.J., U.S.A.*

1895, April 30. Elster, Julius, Ph.D. 6, *Lessingstrasse, Wolfenbüttel.*

1900, April 24. Ewing, James Alfred, C.B., M.A., LL.D., F.R.S., Director of Naval Education to the Admiralty. *Royal Naval College, Greenwich.*

1889, April 30. Farlow, W. G., Professor of Botany. *Harvard College, Cambridge, Mass., U.S.A.*

1900, April 24. Forsyth, Andrew Russell, M.A., Sc.D., F.R.S., Sadlerian Professor of Pure Mathematics. *Trinity College, Cambridge.*

1892, April 26. Fürbringer, Max, Professor of Anatomy. *Universität, Heidelberg.*

1900, April 24. Geikie, James, D.C.L., LL.D., F.R.S., Murchison Professor of Geology and Mineralogy. *Kilmorle, Colinton Road, Edinburgh.*

1895, April 30. Geitel, Hans. 6, *Lessingstrasse, Wolfenbüttel.*

1894, April 17. Glaisher, J. W. L., Sc.D., F.R.S., Lecturer in Mathematics. *Trinity College, Cambridge.*

1894, April 17. Gouy, A., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics. *Faculté des Sciences, Lyons.*

1900, April 24. Haeckel, Ernst, Ph.D., Professor of Zoology. *Zoologisches Institut, Jena.*

1894, April 17. Harcourt, A. G. Vernon, M.A., D.C.L., F.R.S., V.P.C.S. *St. Clare, Ryde, Isle of Wight.*

1894, April 17. Heaviside, Oliver, F.R.S. *Homefield, Lower Warberry Torquay.*

1892, April 26. Hill, G. W. *West Nyack, N.Y., U.S.A.*

Date of Election

1888, April 17. Hittorf, Johann Wilhelm, Professor of Physics. *Polytechnicum, Münster.*

1892, April 26. Hoff, J. van't, Ph.D., For. Mem. R.S., Professor of Chemistry. 2, *Uhlandstrasse, Charlottenburg, Berlin.*

1892, April 26. Hooker, Sir Joseph Dalton, G.C.S.I., C.B., O.M., D.C.L., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *The Camp, Sunningdale, Berks.*

1869, Jan. 12. Huggins, Sir William, O.M., K.C.B., LL.D., D.C.L., F.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 90, *Upper Tulse Hill, Brixton, London, S.W.*

1892, April 26. Klein, Felix, Ph.D., For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Mathematics. 3, *Wilhelm Weber Strasse, Göttingen.*

1894, April 17. Königsberger, Leo, Professor of Mathematics. *Universität, Heidelberg.*

1892, April 26. Ladenburg, A., Ph.D., Professor of Chemistry. 3, *Kaiser Wilhelm Strasse, Breslau.*

1902, May 13. Larmor, Joseph, M.A., D.Sc., LL.D., Sec. R.S., F.R.A.S. *St. John's College, Cambridge.*

1892, April 26. Liebermann, C., Professor of Chemistry. 29, *Matthäi-Kirch Strasse, Berlin.*

1887, April 19. Lockyer, Sir J. Norman, K.C.B., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Science School, South Kensington, London, S.W.*

1902, May 13. Lodge, Sir Oliver Joseph, D.Sc., LL.D., F.R.S., Principal of the University of Birmingham. *The University, Birmingham.*

1900, April 24. Lorentz, Henrik Anton, Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics. *Hooigracht, 48, Leyden.*

1892, April 26. Marshall, Alfred, M.A., Professor of Political Economy. *Balliol Croft, Madingley Road, Cambridge.*

1892, April 26. Mascart, E. E. N., For. Mem. R.S., Membre de l'Institut, Professor at the Collège de France. 16, *Rue Christophe Colomb, Paris, &c.*

1901, April 23. Metschnikoff, Élie, D.Sc., For. Mem. R.S. *Institut Pasteur, Paris.*

Date of Election.

1895, April 30. Mittag-Leffler, Gösta, D.C.L. (Oxon.), For. Mem. R.S., Professor of Mathematics. *Djursholm, Stockholm.*

1894, April 17. Murray, Sir John, K.C.B., LL.D., D.Sc., F.R.S. *Challenger Lodge, Wardie, Edinburgh.*

1894, April 17. Neumayer, Professor G., For. Mem. R.S., Director of the Seewarte. *Hohenzollern Strasse, 9, Neustadt an der Haardt, Germany.*

1887, April 19. Newcomb, Simon, For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Mathematics and Astronomy. 1620, *P Street, Washington, D.C., U.S.A.*

1902, May 13. Osborn, Henry Fairfield, Professor of Vertebrate Palæontology. *Columbia College, New York, U.S.A.*

1894, April 17. Ostwald, W., Professor of Chemistry. *Groszbothen, Kgr. Sachsen.*

1899, April 25. Palgrave, R. H. Inglis, F.R.S., F.S.S. *Henstead Hall, Wrentham, Suffolk.*

1894, April 17. Pfeffer, Wilhelm, For. Mem. R.S., Professor of Botany. *Botanisches Institut, Leipsic.*

1892, April 26. Poincaré, H., For. Mem. R.S., Membre de l'Institut, Professor of Astronomy. 63, *Rue Claude Bernard, Paris.*

1892, April 26. Quincke, G. H., For. Mem. R.S., Professor of Physics. *Universität, Heidelberg.*

1899, April 25. Ramsay, Sir William, K.C.B., Ph.D., F.R.S., Professor of Chemistry in University College, London. 12, *Arundel Gardens, Notting Hill, London, W.*

1886, Feb. 9. Rayleigh, John William Strutt, Lord, O.M., M.A., D.C.L. (Oxon.), LL.D. (Univ. McGill), P.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Terling Place, Witham, Essex.*

Date of Election.

1900, April 24. Ridgway, Robert, Curator of the Department of Birds, U.S. National Museum. *Brookland, District of Columbia, U.S.A.*

1897, April 27. Roscoe, Sir Henry Enfield, B.A., LL.D., D.C.L., F.R.S., V.P.C.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 10, *Bramham Gardens, Earl's Court, London, S.W.*

1902, May 13. Scott, Dukinfield Henry, M.A., Ph.D., F.R.S., F.L.S. *East Oakley House, Oakley, Hants.*

1892, April 26. Sharpe, R. Bowdler, LL.D., F.L.S., F.Z.S. *British Museum (Natural History), Cromwell Road, London, S.W.*

1892, April 26. Solms, H., Graf zu, Professor of Botany. *Universität, Strassburg.*

1886, Feb. 9. Strasburger, Eduard, D.C.L., For. Mem. R.S., Professor of Botany. *Universität, Bonn.*

1895, April 30. Suess, Eduard, Ph.D., For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Geology. 9, *Africaner gasse, Vienna.*

1892, April 26. Thiselton-Dyer, Sir W. T., K.C.M.G., C.I.E., M.A., Sc.D., Ph.D., LL.D., F.R.S. Lately Director Royal Botanic Gardens, Kew. *The Ferns, Witcombe, Gloucester.*

1895, April 30. Thomson, Joseph John, M.A., Sc.D., F.R.S., Professor of Experimental Physics. 6, *Scrope Terrace, Cambridge.*

1894, April 17. Thorpe, T. E., C.B., Ph.D., D.Sc., LL.D., F.R.S., V.P.C.S. *Government Laboratory, Clement's Inn Passage, Strand, London, W.C.*

1894, April 17. Turner, Sir William, K.C.B., M.B., D.C.L., F.R.S., F.R.S.E., Professor of Anatomy. 6, *Eton Terrace, Edinburgh.*

1886, Feb. 9. Tylor, Edward Burnett, D.C.L. (Oxon), LL.D. (St. And. and McGill Univs.), F.R.S., Professor of Anthropology. *Museum House, Oxford.*

1894, April 17. Vines, Sidney Howard, M.A., D.Sc., F.R.S., Sherardian Professor of Botany. *Headington Hill, Oxford.*

Date of Election.

1894, April 17. Warburg, Emil, Professor of Physics. *Physikalisches Institut, Neue Wilhelmstrasse, Berlin.*

1894, April 17. Weismann, August, Professor of Zoology. *Universität, Freiburg i. Br.*

1888, April 17. Zirkel, Ferdinand, For. Mem. R.S., Professor of Mineralogy. *Thralstrasse,* 33, *Leipsic.*

CORRESPONDING MEMBER.

1850, April 30. Harley, Rev. Robert, Hon. M.A.(Oxon), F.R.S., F.R.A.S., Hon. Memb. R.S. Queensland. *Rosslyn, Westbourne Road, Forest Hill, London, S.E.,* and *The Athenæum Club, London, S.W.*

Awards of the Wilde Medal under the conditions of the Wilde Endowment Fund.

1896. Sir GEORGE G. STOKES, Bart., F.R.S

1897. Sir WILLIAM HUGGINS, K.C.B., F.R.S.

1898. Sir JOSEPH DALTON HOOKER, G.C.S.I., C.B., F.R.S.

1899. Sir EDWARD FRANKLAND, K.C.B., F.R.S.

1900. Rt. Hon. LORD RAYLEIGH, F.R.S.

1901. Dr. ÉLIE METSCHNIKOFF, For.Mem.R.S.

1903. Prof. FRANK W. CLARKE, D.Sc.

1905. Prof. CHARLES LAPWORTH, LL.D., F.R.S.

Awards of the Dalton Medal.

1898. EDWARD SCHUNCK, Ph.D., F.R.S.

1900. Sir HENRY E. ROSCOE, F.R.S.

1903. Prof. OSBORNE REYNOLDS, LL.D., F.R.S.

Awards of the Premium under the conditions of the Wilde Endowment Fund.

1897. PETER CAMERON.

1898. JOHN BUTTERWORTH, F.R.M.S.

1899. CHARLES H. LEES, D.Sc.

1900. Prof. A. W. FLUX, M.A.

1901. THOMAS THORP.

THE WILDE LECTURES.

1897. (July 2.) "On the Nature of the Röntgen Rays." By Sir G. G. STOKES, Bart., F.R.S. (*28 pp.*)

1898. (Mar. 29.) "On the Physical Basis of Psychical Events." By Sir MICHAEL FOSTER, K.C.B., F.R.S. (*46 pp.*)

1899. (Mar. 28.) "The newly discovered Elements; and their relation to the Kinetic Theory of Gases." By Prof. WILLIAM RAMSAY, F.R.S. (*19 pp.*)

1900. (Feb. 13.) "The Mechanical Principles of Flight." By the Rt. Hon. LORD RAYLEIGH, F.R.S. (*26 pp.*)

1901. (April 22.) "Sur la Flore du Corps Humain." By Dr. ÉLIE METSCHNIKOFF, For.Mem.R.S. (*38 pp.*)

1902. (Feb. 25.) "On the Evolution of the Mental Faculties in relation to some Fundamental Principles of Motion." By Dr. HENRY WILDE, F.R.S. (*34 pp., 3 pl.*)

1903. (May 19.) "The Atomic Theory." By Professor F. W. CLARKE, D.Sc. (*32 pp.*)

1904. (Feb. 23.) "The Evolution of Matter as revealed by the Radio-active Elements." By FREDERICK SODDY, M.A. (*42 pp.*)

1905. (Feb. 28.) "The Early History of Seed-bearing Plants, as recorded in the Carboniferous Flora." By Dr. D. H. SCOTT, F.R.S. (*32 pp., 3 pl.*)

1906. (March 20.) "Total Solar Eclipses." By Professor H. H. TURNER, D.Sc., F.R.S. (*32 pp.*)

1907. (February 18) "The Structure of Metals." By Dr. J. A. EWING, F.R.S., M.Inst.C.E. (*20 pp., 5 pls., and 5 text-figs.*)

1908. (March 3.) "On the Physical Aspect of the Atomic Theory." By Professor J. LARMOR, Sec. R.S. (*54 pp.*)

LIST OF PRESIDENTS OF THE SOCIETY.

Date of Election.	
1781.	PETER MAINWARING, M.D., JAMES MASSEY.
1782-1786.	JAMES MASSEY, THOMAS PERCIVAL, M.D., F.R.S
1787-1789.	JAMES MASSEY.
1789-1804.	THOMAS PERCIVAL, M.D., F.R.S.
1805-1806.	Rev. GEORGE WALKER, F.R.S.
1807-1809.	THOMAS HENRY, F.R.S.
1809.	*JOHN HULL, M.D., F.L.S.
1809-1816.	THOMAS HENRY, F.R.S.
1816-1844.	JOHN DALTON, D.C.L., F.R.S.
1844-1847.	EDWARD HOLME, M.D., F.L.S.
1848-1850.	EATON HODGKINSON, F.R.S., F.G.S.
1851-1854.	JOHN MOORE, F.L.S.
1855-1859.	Sir WILLIAM FAIRBAIRN, Bart., LL.D., F.R.S.
1860-1861.	JAMES PRESCOTT JOULE, D.C.L., F.R.S.
1862-1863.	EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
1864-1865.	ROBERT ANGUS SMITH, Ph.D., F.R.S.
1866-1867.	EDWARD SCHUNCK, Ph.D., F.R.S.
1868-1869.	JAMES PRESCOTT JOULE, D.C.L., F.R.S.
1870-1871.	EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
1872-1873.	JAMES PRESCOTT JOULE, D.C.L., F.R.S.
1874-1875.	EDWARD SCHUNCK, Ph.D., F.R.S.
1876-1877.	EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
1878-1879.	JAMES PRESCOTT JOULE, D.C.L., F.R.S.
1880-1881.	EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
1882-1883.	Sir HENRY ENFIELD ROSCOE, D.C.L., F.R.S.
1884-1885.	WILLIAM CRAWFORD WILLIAMSON, LL.D., F.R.S.
1886.	ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
1887.	BALFOUR STEWART, LL.D., F.R.S.

*Elected April 28th; resigned office May 5th.

1888-1889. Osborne Reynolds, LL.D., F.R.S.
1890-1891. Edward Schunck, Ph.D., F.R.S.
1892-1893. Arthur Schuster, Ph.D., F.R.S.
1894-1895. Henry Wilde, F.R.S.
1896. Edward Schunck, Ph.D., F.R.S.
1897-1898. James Cosmo Melvill, M.A., F.L.S.
1899-1900. Horace Lamb, M.A., F.R.S.
1901-1902. Charles Bailey, M.Sc., F.L.S.
1903-1904. W. Boyd Dawkins, M.A., D.Sc., F.R.S.
1905-1906. Sir William H. Bailey, M.I.Mech.E.
1907-1908. Harold Baily Dixon, M.A., F.R.S.

then in ether, the effect could not be removed. By boiling, however, in a dilute solution of caustic soda for a few minutes, all trace of the action of the oxalic acid vanished, without any tendering of the fabric being noticeable. This observation precluded, therefore, the possibility of the formation of oxycellulose. It almost appeared as if some of the oxalic acid had combined with the cellulose to form an oxalate, which, by analogy with the nitrates, acetates, &c., would show the characteristic behaviour towards dyestuffs. I was unable, however, to detect oxalic acid in the caustic soda extract of the treated fabric, and for some time gave up the work.

On resuming my investigation into the subject, I was led to test the caustic soda extract of the treated fabric for formic acid, which was found to be present. It would therefore appear that in drying the oxalic acid had undergone decomposition into carbonic acid and formic acid, which latter had in the nascent state combined with the cellulose. Attempts to determine quantitatively the actual amounts of cellulose formiate produced were not satisfactory, the results obtained being very irregular. In any case, however, the amounts found appear to be only small.

This explanation of the action of oxalic acid on cellulose becomes all the more probable from the results of further experiments with malonic acid and hexylmalonic acid, both of which behave like oxalic acid, and this would be expected from their behaviour on heating. In the saponification product from cotton treated with malonic acid, it was possible to detect acetic acid.

Succinic acid, on the other hand, does not show any alteration in the cellulose, nor does glutaric acid.

It would appear, therefore, that the action of oxalic acid on cellulose simply constitutes one example of a general mode of formation of acidyl celluloses.

Professor KNECHT also exhibited specimens of commercial metallic titanium and commercial silicon.

In a dicussion which took place later Mr. JULIUS HÜBNER

said that, in a paper read before this Society in November last, on "New Reactions for the Characterisation of Mercerised Cotton," he pointed out that the action of other salts and acids in the presence of very small quantities of iodine was still under investigation. He had found in the meantime that characteristic reactions on mercerised cotton were produced by a large number of acids and salts in the presence of iodine. Among others he wished to mention here oxalic acid, citric acid, formic acid, and tartaric acid. With all these acids mercerised cotton exhibited a greater reactivity than did ordinary cotton. Glacial acetic acid was found to be less suited for this purpose.

RECENT ADDITIONS TO THE LIBRARY.—Continued.

Haarlem.—Société Hollandaise des Sciences. Œuvres complètes de C. Huygens. Tome 11e. 1908.

London.—Institution of Electrical Engineers. The Kelvin Lecture: The Life and Work of Lord Kelvin. By S. P. Thompson. (1908.)

—— **Meteorological Office.** Barometric Gradient and Wind Force. Report... By E. Gold. 1908.

—— —— The Observer's handbook. By R. H. Scott. New ed. 1908.

Middelburg.—Zeeuwsch Genootschap der Wetenschappen. Catalogus der Numismatische Verzameling...door M. G. A. de Man. 1907.

Oxford.—University Observatory. Astrographic Catalogue, 1900-0. Oxford Section, Dec. + 24° to + 32°... [prepared] under the direction of H. H. Turner... Vols. 3, 4. 1907-8.

Paoli (G. C.). Idea dell' Universo... di G. C. Paoli. Note. 1907-8.

Raymond (G. L.). The Aztec God and other Dramas. 3rd ed. 1908.

—— Ballads and other Poems. 3rd ed. 1908.

—— A Life in Song. 3rd ed. 1908.

—— The Psychology of Inspiration. 1908.

Transvaal.—Agent General for. Geodetic Survey of South Africa. Vol. 5. Reports on the... survey of the Transvaal and Orange River Colony, executed by Col Sir W. G. Morris... 1908.

Washington.—Bureau of American Ethnology. Antiquities of the Upper Gila and Salt River Valleys in Arizona and New Mexico. By W. Hough. 1907.

Venice.—Reale Istituto Veneto di Scienze, Lettere ed Arti. Osservazioni meteorologiche e geodinamiche eseguite nell' anno 1906 nell' Osservatorio del Seminario Patriarcale di Venezia. 1907.

NEW EXCHANGES.

Manchester.—Municipal School of Technology. Journal.

Mérida de Yucatan.—Observatorio Meteorologico. Boletin mensuel.

Roma.—Società Italiana per il Progresso delle Scienze. Atti.

And the usual Exchanges and Periodicals.